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BOOKS, ETC., RECEIVED.

A Manual of the Diseases of the Camel. By John Henry Steel, V.S., A.V.D., F.R.C.V.S., F.Z.S., &c.

The Quarterly Journal of Veterinary Science in India, October, 1890.

Indian Veterinary Journal (published in Urdu).

The Journal of Comparative Medicine and Veterinary Archives, February, 1891.

Polymastism, with Reference to Mammæ Erraticæ and the Development of Neoplasms from Supernumerary Mammary Structures. By W. Roger Williams, F.R.C.S., &c.

The Johns Hopkins Hospital Bulletin, Vol. II, No. 10.

Vinton's (late Morton's) Agricultural Almanac, 1891, *a Year Book for Farmers and Landowners.*

Agricultural Students' Gazette, December, 1890.

The Dog Owners' Annual for 1891.

The following foreign publications are to hand:—*Annales de Medecine Vétérinaire,* February, 1891; *Oesterreichische Monatsschrift für Thierheilkunde, und Revue für Thierheilkunde und Thierzucht,* March; *Archivio per le Scienze Mediche, La Clinica Veterinaria* (Milan); *Zeitschrift für Veterinärkunde mit besonderer Berücksichtigung der Hygiene,* Berlin, February, 1891; *Recueil de Médecine Vétérinaire,* December, 1890; *Zeitschrift für Fleish- und Milchhygiene,* October; *Verlag des Archivs für Animal, Nahrungsmittekunde* (Cologne, Rhine), *Bulletin et Mémoires de la Société Centrale,* and *Central-Zeitung für das Kochsche Heilverfahren* (Berlin), February 1.

Papers received: *Mark Lane Express, North British Agriculturist, Land and Water, The Field, Live Stock Journal, Bell's Weekly Messenger, Agricultural Gazette, Farmers' Gazette,* Dublin; *Isle of Wight Observer and Chronicle, The Isle of Wight County Press, Edinburgh Evening Dispatch, The Gazette,* Montreal; *The Essex County Chronicle,* and *The Denver Times,* U. S., January 2nd.

It is particularly requested that in future the British and Foreign Exchange Journals be directed to the EDITORS of the *Veterinarian,* 22½, Bartholomew Close, London, E.C.

NOTICE TO CORRESPONDENTS.

The report of the Central Veterinary Medical Society arrived too late for insertion in the present issue.

To ensure the publication of Cases and Communications in the coming Number they should arrive by the 15th of the Month.

LETTERS, REPORTS OF CASES, ETC., MAY BE DIRECTED EITHER TO PROFESSOR SIMONDS, J.P., RYDE, ISLE OF WIGHT, OR TO THE "EDITORS," AT THE ROYAL VETERINARY COLLEGE, OR TO THE CARE OF ADLARD & SON, PRINTERS, 22½, BARTHOLOMEW CLOSE, LONDON, E.C.

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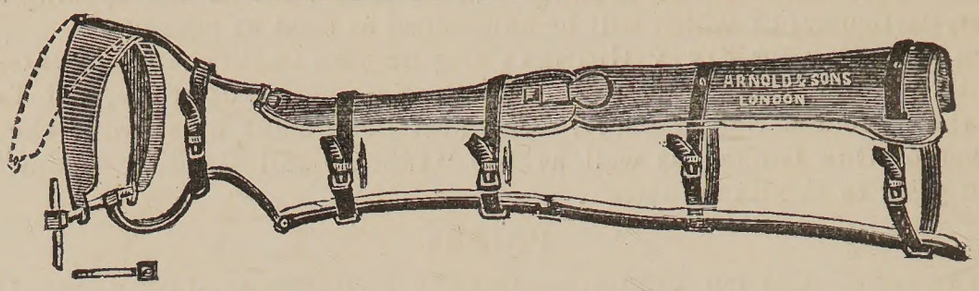

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Communications and Cases.

A FEW MORE THOUGHTS ON INFLUENZA. WHAT IS IT?

By Thomas Greaves, F.R.C.V.S., Manchester.

In my former paper on this subject I propounded the above question; to my thinking it has not yet been satisfactorily answered. I said the disease was attributed to atmospheric influence, but what that atmospheric influence exactly is does not appear. That it is a pestilence of a malignant and subtle nature is admitted, but whether it depends on microbes floating in the air, is generated spontaneously, or emanates from different animals affected, is not to my mind at all clear; or whether it is some poisonous emanation from out of the earth which permeates the air, and is carried by currents all over the surface of the land, are matters not yet clearly explained. I had hoped our scientists and men of special training would have defined the exact nature of what this atmospheric influence really is. We find the horses affected in one locality to be far more virulently attacked than horses in other localities. I think this goes a long way to prove that the atmosphere is surcharged more in one place than in another; we find horses that have not been out of their stables for weeks or months attacked, and that they have it in virulent form, notwithstanding they had not been near other horses, or any other horses been near them. I am now speaking of this one form of influenza commonly called "pink-eye,"

where we recognise an accelerated and feeble pulse, cold ears, eyes closed, the temperature increased, loss of appetite, swollen limbs and sheath, stiffness and weakness, &c.

We may meet with influenza at all seaons of the year. I have seen numbers of outbreaks during the last fifty-six years, but what I want to know is, what is the true nature and cause of this special disease called "pink-eye"? It affects horses in clean, sweet, well-ventilated stables. I do not find that any kind of disinfectant is of any avail, but I feel pretty clear on this point, that those horses which have been living in ill-ventilated stables, breathing an atmosphere impregnated with foul emanations from drains, are much more liable to contract the disease, and that when attacked they succumb sooner than others. During this winter we have been visited by this epizoötic, which in places has been very destructive of life; during this winter also we have had a good deal of intensely cold and foggy weather, giving the horses deathly chills, which no doubt has added greatly to the increase of the death-rate. We know it is a fact that if we take a mouse and drop a few drops of ether upon it, it soon dies, owing to rapid evaporation robbing it of its caloric; if we wrap a blanket saturated with ether round a man the effect is the same, the chill is so great that he will shortly be deprived of that portion of heat essential to existence. Now during December, January, and a part of February the cold of most days was very severe, and the fog was very dense; this then enveloped and wrapped the horse like being in a wet blanket, and would thus rob him of caloric. But it did another thing as well; in many cases there was an inability to breathe, the process of inspiration and expiration was next to impossible. This difficulty was in some cases gradual, in others rapidly assumed; in short, a state of apnœa or asphyxia took place, and death was the result.

Now we all remember the great losses incurred some years since amongst those grand, beautiful, valuable and prize cattle that died or were destroyed whilst being exhibited in the Agricultural Hall at the great show during a fog of unusual density. There were no microbes there, there was no diseased blood there, no "pink-eye" there, but simply and purely an inability to breathe, at the same time robbed of their natural caloric; there they stood gasping for their breath, and struggling for life, exactly as I have seen young, strong, valuable horses struggling for life, and die before my eyes this winter. The *post-mortem* appearance is exactly the same, unless in some horses in whom there might be

more or less disease of old standing in the lungs or liver, or in both. In some cases we may find a naturally weak heart, the walls of the heart thin; still both it and every vital organ in the body being in a perfectly sound and healthy condition. In such cases death could only be attributable to failure of the heart's action, ending fatally in syncope. We find black blood heaped up in the ventricles, auricles, and large blood-vessels in the one case as in the other, as if endosmosis and exosmosis had been entirely suspended.

I am about instituting some experiments with pure oxygen gas mixed in the air the animals breathe, with a view to stimulate the process of endosmosis and exosmosis. I do not profess to have any special talent, nor have I had any special training to carry out these investigations or researches, but since our scientists and those who have had special training do not seem disposed to undertake it, I can only do my best and hope for a beneficial result.

FRACTURED LOWER JAW, OPERATION AND RECOVERY.

By William Littlewood, M.R.C.V.S., Sanitary Administration, Cairo.

On February 3rd, 1890, grey horse No. 24, of Cairo City Police, whilst escorting the postal van at a trot, ran against it, fracturing his lower jaw in two places, one at the symphysis, the other between second and third right incisors, knocking out two teeth; the two pieces of the left jaw were quite pendulous, and could easily have been removed with a little dissection. The case was shown to Dr. Milton, of Kass el Aine Hospital, Cairo, who kindly consented to operate.

The next day the horse was cast, chloroformed, and the three pieces of the jaw were brought together; a hole was then drilled through them, and a strong piece of wire passed through the hole, brought round in front of the bone below the incisors, the mucous membrane being cut through to the bone, so as to allow the wire to embed itself; wire was then fastened and wound stitched up.

Animal was under chloroform for about one hour, and on coming round great difficulty was experienced in preventing it from injuring its jaw; was then allowed to rise and placed in a stall, standing on pillow reins; being fed

by hand, great care had to be observed in giving him water, as on every possible occasion he rubbed the jaw. On the fourth day after the operation symptoms of congested lungs became apparent, the animal was then placed in a loose box, stimulants given, and sides blistered. At end of a week was better, in a month was exercised, at ten months was doing light duty, snaffle bridle only being used; is now doing ordinary patrol with military bits and bridle; suffers no inconvenience from the wire embedded and left in the jaw.

"ARE BLISTERS HURTFUL OR BENEFICIAL?"

By Professor WILLIAMS.

To the Editor of the 'North British Agriculturist.'

"SIR,—Professor McCall carefully avoids answering my proposal that this matter be put to the test, and still goes on reiterating well-known arguments in support of his opinions.

"He says that in *my* opinion counter-irritation is not beneficial in chest diseases. This is not a matter of mere opinion with me, but one which has been forced upon me by the results of a long experience. Like Professor McCall, I was once an advocate of this kind of treatment, but I soon found that it was not a successful one. I therefore altered my line of treatment, with a result which has been most gratifying to me as well as profitable to my clients for more than twenty-five years.

"Professor McCall says that he had anticipated a reply from me, proving that I 'knew not only more about "reflex action" than any other of my contemporaries, but that I could and would prove to them that what they did know was founded on error.' This is a compliment to me—if meant as such—which I am sure I don't deserve. I will say this much, however, that if you have an acutely inflamed surface, and apply an irritant to the skin opposite that surface, as is done in the treatment of pleurisy, you will most assuredly increase the original inflammation. This can easily be proved by causing a superficial inflammation around a chronically diseased joint, and dissecting the parts, say, three days after, when it will be found that the skin, subcutaneous tissues, and the bones if sufficiently near the surface, such as those of the hock, pastern, &c., are all involved in the inflammatory action thus induced. In this manner counter-irritants are beneficial in chronic inflammations; but if the process be an acute one, and sufficiently acute, when involving an internal organ, as to endanger life, does it not seem irrational to increase the condition of acuteness?

"With regard to the application of irritants to the skin of the chest in pure pneumonia or inflammation of the substance

of the lungs, I cannot see any direct vascular connection between the parts. This I know, however, that when I did apply counter-irritants in the treatment of pleurisy or pleuro-pneumonia, the condition of hydrothorax ending fatally was not an uncommon one, but that now this dropsy of the chest is almost unknown, although pleurisy and pleuro-pneumonia are as rife as ever.

"Within the last few weeks a veterinarian whom I know very well, but whose name I am not at liberty to mention, had several cases of chest influenza—pleurisy and pleuro-pneumonia—in a stud of horses. Several of these being unwell, a consultation was held, and the 'consultant' at once ordered the whole lot to have mustard freely applied to the sides. Well, sir, what was the result? Twenty-five per cent. of these had hydrothorax the next day, which in every case terminated fatally, a result which was not expected by the first attending practitioner before the mustard was applied. As Professor McCall has quoted a case where his treatment was successful, I may say that on Wednesday last, 21st inst., we were called to attend a mare in Leith with pleuro-pneumonia, high fever, temperature 106° F., &c. Yesterday the temperature was 104° F., and breathing much better; and to-day, 27th inst., her temperature is normal, and all signs of distress in the breathing have disappeared, the lungs themselves clearing up, and the only palpable signs of ill-health are a still diminished appetite and debility.

"Professor McCall, who is one of the editors of the *Veterinarian*, informs us that he has arranged to transfer this correspondence to that journal. I do not know, Mr. Editor, whether he has your consent or not, he has certainly not asked me for mine; but as the old journal seems to be about *moribund*, I do not object to this arrangement, to put a *little go* into the 'favourite.' At the same time I think that, this being a matter of great public interest, the discussion should be continued in a journal like yours, which is read not only by veterinarians, but by medical men and agriculturists, who have sufficient knowledge to follow the arguments, and from them to form their own opinions.—I am, &c."

ARE BLISTERS HURTFUL OR BENEFICIAL IN ACUTE DISEASES OF THE CHEST?

By Professor McCall.

An Address Delivered at the Meeting of the North of England Veterinary Medical Association.

Gentlemen,—When I received the information through your Secretary that it was the desire of the members of this Society that

I should open to-day's meeting by the introduction of a subject for discussion, I naturally set my mind to seek out one which would be of interest to all, and on which all might express an opinion. Observing in the pages of the *North British Agriculturist*, as I dare say many of you also had, that a discussion had arisen on the subject of "Counter-irritation in Chest Affections," and that that discussion had, to say the least, been abruptly terminated, I thought it would not be out of place in making it the theme of my remarks to-day. It cannot be said that the subject of counter-irritation in chest diseases is one of no special interest to the physician and his patient, or that it is immaterial whether the veterinarian avails himself of such aids in the treatment of chest diseases, or abstains from their application. That counter-irritants have a positive action and influence on the subject to which they are applied, and that for good or bad, is unquestionable; and now that there are members in our ranks (whether few or many I cannot say) openly declaring that they are destructive instead of helpful aids in the treatment of almost if not all chest affections, we as a profession can neither be still nor remain neutral observers. Nay, I hold that by the publicity which has been given to the discussion the time has now come when, in order to right ourselves in the eyes of the public (if for no higher reason), it is imperative that each member of the profession should give expression to the results of his experience, and the conscious belief he entertains as to the value or otherwise of counter-irritation in chest diseases. If bad they do, they must be discontinued, and honour bestowed on the member of our profession who has most openly declared against them, namely, Professor Williams. But, on the other hand, even supposing those who advocate their use be charged (and not unjustly by their opponents) as not at one as to the *modus operandi* or manner in which they act for good, yet, if convinced that good results, they must defend their position at all hazards. With these I consider necessary observations to the introduction of the subject I shall now pointedly refer to some of the remarks Professor Williams has made in his writings regarding counter-irritation. In his work on 'The Principles and Practice of Veterinary Medicine' he says, "Irritation of the sides and breast is injurious in *all* instances of chest diseases in the horse, excepting those cases in the second stage of pleurisy, and where the horse neither gets better nor worse for several days—'hangs fire,' as it is commonly termed." Again, "The application of blisters causes pain, increases the fever, and cannot and does not remove the internal inflammation. If applied to the sides, they impede the respiratory movements by the pain they cause, and thus add to the distress and suffering of the animal." Now these are serious charges, and if they could be substantiated they might justify the abolition of counter-irritants in chest diseases; but when I carefully peruse the work from which I have made these extracts, in the hope of finding valid reasons for the condemnation thus passed on them, I fail in discovering even a vestige of proof. Professor Williams twits the advocates of counter-

irritation with differing in their views as to how counter-irritants act and at what stage of the inflammation they should be applied ; and yet without experimental evidence or proof of any kind he hesitates not to state that "counter-irritation is injurious, with one exception, in *all* chest affections." He also writes, "I stand almost alone in my condemnation of the prevailing method of treating such diseases, and at the present time this Institution is the only college in Great Britain in which counter-irritation in chest affections is condemned." Might I ask, is this written in pain and disappointment, or in pride and, I had almost said, arrogance? I hope the former ; but if so, has he not himself to blame if he stands almost alone in the condemnation of counter-irritants? Students, out of respect and love for their teacher, may follow his lead, but the time comes when even they will refuse to believe in that which they cannot understand ; and as Professor Williams has yet to explain and prove how counter-irritation acts prejudicially in chest diseases, need he wonder that probably ninety per cent. or even more of his professional brethren still remain the advocates of counter-irritation and unconverted by what he has said? Judging from what has been written by those who decry counter-irritation, it is fraught with danger to offer any remarks by way of explanation as to the manner in which counter-irritants act ; and unless the advocates of counter-irritation can reconcile the various views expressed or entertained, the opponents of counter-irritation proclaim "this is another argument which tells against blisters." If so, is it not a very weak one? But what are the advocates of counter-irritation under given conditions to say of their opponents who condemn counter-irritants and offer no explanation or proof? It is clear, however, that no progress will be made in deciding whether blisters are beneficial or hurtful in the treatment of chest diseases until their advocates and opponents cease to dogmatise, and resort to mutual explanations and proof. As it is Professor Williams has not only called in question the action and utility of counter-irritants, but who has also proclaimed that they are hurtful in chest affections, therefore it is he who should have led evidence and proved to us wherein we were in error ; but as that would entail the adjournment of our meeting, I am quite prepared to give him the advantage of what I have briefly to say in support of counter-irritation.

Firstly.—How then does the counter-irritant mustard act on the chest, and produce a beneficial result in congestions and inflammations of lung-tissue? To answer this question I must refer to the anatomical structure of the blood-vessels.

The nervous apparatus for the control of the calibre or size and movements of the minute arteries of the body is at once wonderfully simple and infinitely complex. We may call it the "vaso-motor" mechanism. And first and foremost it must be distinctly understood that this vaso-motor mechanism consists of two separate and independent parts, each controlling its own districts in its own way. The first part goes by the rather misleading name of "vaso-

constrictor" mechanism, which, to say the least, is rather awkward, for it may at one time bring about constriction of the blood-vessels, and at another time dilatation of the same.

Its head office, or chief "centre," is located in the "medulla oblongata;" and there are also, but less important, "centres" in the spinal cord. From these centres "efferent" or outgoing nerve-fibres run through the so-called "sympathetic system" to the vast arterial districts of the skin and alimentary canal. During the greater part of time these fibres are engaged or employed in carrying *out* from the "centres" impulses just strong enough to keep the walls of the small arteries (which are formed essentially of circular muscular fibres) of each district in a state of moderate constriction, which we call "tone." But from time to time as occasion arises the "centres" are induced either to send out extra strong impulses, bringing about extra constriction and consequent anæmia and pallor of the surface, or to lessen or stop the issue of the usual and normal impulses, and in this way bring about dilatation of the walls of the vessels, and consequent redness or blushing of the surface.

The "centres" are induced to make these changes in their usual behaviour either by messages from the *brain* (one "emotion," for example, causes pallor or a death-like whiteness of the face, while another "emotion" calls up a crimson blush), or from almost *any part of the body, including the skin itself.*

In the latter case, for example, when we put a blister to the skin there is a message to the "centres," an occurrence in the centres, and perhaps a partial or total stop in the issue of the usual impulses to the small arteries of the portion of skin. There is a triple occurrence called a "reflex" action. The skin blushes. This first part of the "vaso-motor" mechanism—this so-called "vaso-constrictor" mechanism—is in the main the instrument to be played upon in the practice of counter-irritation. The other part, the "vaso-dilator" mechanism, we need not consider at length, as it is more immediately concerned in regulating the dilatation of vessels in glands during the periods of active secretion.

Much more might be stated—indeed, less could not be said; but what has been stated rests on a proven basis. But to proceed, we have now submitted to our care two horses, labouring under an attack of pneumonia, in the stage of congestion and exudation; and we will suppose that the one animal is younger and more robust than the other, or the attack is more aggravated and involves a larger surface, or the lung-tissue is not so healthy in the second as in the first horse. To start with, we know we have two very different patients to deal with, although labouring under the same malady; and our internal remedies must be different. To the chest of both, however, a mustard cataplasm is applied, in each one pound of mustard, well rubbed in, paper above, body-sweller over all, fitting exactly, being laced on, but not applied tight. In half an hour the more robust animal is showing considerable uneasiness, and even lying down and getting up; and he repeats this several times. Two hours have passed over, the perspiration has ceased,

the pain the result of the irritant has disappeared, the pulse and respirations are quicker, the temperature 1° up, and the sides are tender to touch. In from six to twelve hours the temperature, pulse, and respirations are as low as they were before the application of the mustard, the sides of the chest are swollen from effusion ; and from this time onwards we have a gradual improvement and an ultimate recovery. In our weaker subject the symptoms exhibited on the application of the mustard are much the same as in the first, namely, increase of pulse, temperature, and breathing; but in some subjects such as this one the pain consequent on the application of the mustard is very slight indeed ; but whether or not, one thing is observable, namely, the surfaces to which the mustard has been applied are not in the least tender to touch or swollen from effusion—in other words, the mustard has failed in exciting an inflammation of the skin. We, however (not that it is our practice), re-apply the mustard, and still the result is no greater a success ; and we shall also suppose that this subject does not recover, but eventually dies. Now, why have we in the case of the one animal a marked local inflammation with its attendant results, and in the other no local inflammation worthy of the name? As results of the application of mustard to the skin in health, and in not less than 90 per cent. of chest affections, we have determination and congestion of blood in the blood-vessels of the skin to the extent of the surface of skin subjected to the action of the irritant, and as an after effect, effusion into the cellular tissue under the skin. Now this determination of blood is primarily brought about by the action of the mustard on the sensory nerves of the skin, and through the "vaso-motor centre" in the medulla, on the vaso-constrictor and vaso-dilator nerves, regulating the contractions and dilatations of the blood-vessels of the skin.

In our first animal the telegraphic message flashed by the sensory nerves of the skin to the head office (the vaso-motor centre in the medulla) is instantly responded to, and an influx of blood and a copious effusion is the response. That influx of blood brings with it pain, increase of temperature and breathing ; but it is only temporary, and subsides in a short time, to be replaced by freedom in breathing, slowing of the heart's movements, decline of temperature, gradual subsidence of the congestion and inflammation of lung-tissue, and a return to normal cell action and nutritive activity.

Apparently the conditions which lead to dilatation and accumulation of blood in the vessels of the skin covering the chest-wall, tend to promote a return of contractility in the dilated and engorged blood-vessels of the lungs, and the mechanism of reflex action, and the direct experiments of Zulzer and others all point in this direction. Zulzer found that when cantharides collodion was painted repeatedly over the back of a rabbit on one side for a number of days, the vessels underneath the skin and the superficial layers of muscles were congested, but the deeper layers of the muscles, the thoracic wall, and even the lung itself, were much paler and more anæmic than those of the other side.

In our other patient the counter-irritant has failed in influencing the vaso-motor centre. And why? because the internal inflammation proceeding in the lung is of a more pronounced, more destructive, more overpowering character than in the former case; or, in other words, the paralysed and dilated condition of the walls of the blood-vessels, the accumulation of blood, and effusion of lymph has so seriously disturbed or concentrated nerve-influence at the fountain-head—"vaso-motor centre"—that it has not the power to respond to the local call addressed to it. In other words, the vaso-motor centre is so much occupied by afferent messages from the lung, and perhaps efferent messages to the lung, that it cannot attend to afferent messages from the skin. Surely this is not an unwarrantable explanation; nay, I think it is justified by all we know of the physiology of the nervous system, and of the influence of nerve-power in controlling muscular movement, blood circulation, and brain energy; for where is the individual who can think and speak on two subjects, or act two distinct parts, at the same instant of time? or where is the man who will not change colour under the mental influence of shame, or it may be terror? Professor Williams, as I have previously stated, says, "Counter-irritation is injurious in chest affections because it cannot and does not remove the internal inflammation," and in proof of this he instances a case of pleurisy, where repeated application of counter-irritants had been applied to the sides; and on *post-mortem* he found all the evidences of marked inflammation on the pleura and chest-walls. Now, it entirely depends on what evidence he holds to be confirmatory of the removal of internal inflammation. If, for example, the confirmatory proof must amount to the eye failing to detect any abnormal condition or the presence of abnormal products, then he is quite right; but if counter-irritants on this account are to be discarded, will he mention what agent he can substitute which is not open to the same objection? He cannot, nor any other person; for inflammation once established produces changes that no remedy or combination of remedies can there and then obliterate or sponge out. But although we cannot remove the internal inflammation and its products *instantly*, are there no remedies that can be applied to influence and moderate its course? In ninety-five cases out of one hundred counter-irritants will do this. If, then, they do not remove the internal inflammation, but by establishing a second and more manageable inflammation check the further course of the primary inflammation (as I maintain they do), then they are justly entitled to be called beneficial, and not hurtful agents in the treatment of chest disease.

That *two inflammations* do not flourish together every unbiassed practitioner must have seen cause to admit. When the artificial inflammation established by counter-irritation flourishes, as is evidenced by the heat, pain, and swelling of the chest-wall, the internal inflammation gradually subsides. On the other hand, when the counter-irritant has produced almost no greater effect than its *chemical* action would produce when applied to the chest

of a dead horse, then the internal inflammation may be said to flourish and the external to be of little avail. In this way counter-irritation is not only of value in assisting in checking and moderating internal inflammations, but also in assisting us most materially in prognosticating a favorable or unfavorable termination of the disease. In the treatment of an acute inflammation the relief of pain is most important; and that may be attained in two ways: namely, by the administration of narcotics and anodynes, and by *diverting nerve-energy* from the inflamed part into another channel; and the latter is incomparably better than the former, and of all agents counter-irritants are the best to effect this end.

In proof of this take the very common case of a simple pimple or a boil that has been aggravated by scratching the head off, as also the absorption of some morbific agent. A hard swelling, with great heat and throbbing pain, is the result. The patient is fevered, has no appetite, and, notwithstanding the application of poultices, laxative and narcotic medicine, there is no real relief from pain, or progress made towards a recovery; the "patient hangs fire," to use Professor Williams' own words. We now resort to counter-irritation. At a short distance from the boundary of the inflamed part we apply, with a camel-hair pencil, tincture of iodine, solution of nitrate of silver, or other counter-irritant, in the form of a circle or horseshoe, and what is the result? Within an hour or two the pain has almost ceased, and by a daily application of the counter-irritant for a few days the hard swelling is gradually removed by absorption. We take another case: a horse has an attack of pharyngo-laryngitis. The irritability of the sensory nerves is very marked; he has a craving for food, but experiences great difficulty in swallowing; and either from attempting to swallow too quickly, resulting in spasm of the constrictor muscles of the larynx, or from rapid effusion or œdema of the glottis (for sometimes it is the one and sometimes the other), there ensues a most marked difficulty in breathing, and unless measures be taken to allow air to enter the chest the animal will die. There is one effectual remedy for this condition, namely, tracheotomy; but is there no other? Yes, there is, and one in many such instances quite as potent to relieve the distress in breathing, and that is counter-irritation. Plaster on and rub in two pounds of mustard around the larynx, betwixt the jaws, and down the trachea; and in the bulk of cases relief is experienced, as is demonstrated by the *immediate* cessation of the loud noise and the heaving of the flanks.

That counter-irritation here has not removed the primary inflammation must be patent to all, for there is no sufficient time to do so; but that it has by establishing a local seat of irritation and congestion, and by *concentrating nerve-energy upon it, allayed nerve-irritability* and consequent spasm of muscular fibre in connection with the larynx cannot be denied, and therefore I hold that local or external counter-irritation acts beneficially in the treatment of internal inflammations. Professor Williams also writes, "If you

have an acutely inflamed surface and apply an irritant to the skin opposite that surface as is done in the treatment of pleurisy, you will most assuredly *increase the inflammation.*" Now, is this statement founded on fact? Professor Williams admits that he "cannot see any direct vascular connection between the chest-wall and the lungs," and yet he asserts that if we counter-irritate the chest we will "assuredly increase the original inflammation." Would the Professor not go the length of saying—If you apply a pound of mustard to the chest-wall of a healthy horse you will as a consequence produce inflammation of the lungs? If he will prove that to me, then I will lay down my pen and admit that counter-irritation is worse than useless, a barbarous remedy, and a disgrace to the medical and veterinary profession. But he cannot, and now we come to closer quarters. Can he tell *during life* the *extent* of inflammation existing in the pleura or lungs, and the *exact stage* in which it is? No, he cannot. We now apply a mustard cataplasm to the chest-wall of this same horse, and we shall suppose he dies in twelve hours or twelve days thereafter, or any lesser or longer period. Can Professor Williams, now the book is opened by *post-mortem* examination, point out the lesions produced by the original or primary inflammations, and those added to them by counter-irritation of the skin? No, he cannot. Where, then, I ask, is the proof warranting the charge, the calumny that "if you have an acutely inflamed surface and apply an irritant to the skin opposite that surface, as is done in the treatment of pleurisy, you will most assuredly increase the original inflammation"? But, says the Professor, "this can easily be proved by causing a superficial inflammation around a chronically diseased joint and dissecting the parts, say three days afterwards, when it will be found that the skin, subcutaneous tissue, and the bones, if sufficiently near the surface such as those of the hock, pastern, &c., are all involved in the inflammatory action thus induced." Now, in the meantime granting that what he has thus stated be more or less true as regards the series of changes produced in the various structures mentioned by the application of a cantharidine blister to a chronically diseased joint, does he not perceive that he is begging the question when he says that the same series of changes result when mustard is applied to the chest-wall in inflammation of the lungs, because he has drawn deductions from entirely different premises? The character and stage of inflammation, the anatomical structure and conformation, the relations of blood circulation and nerve distribution are all different; and, moreover, the counter-irritant named (cantharides) is one seldom applied to the chest-wall in the treatment of a lung affection. If the lungs of a horse lay immediately underneath the skin of the chest, as the bones of the hock and the pastern do, and if we were discussing the benefits of counter-irritation in chronic forms of inflammation of the lungs, then Professor Williams' inferences might be pertinent to the subject; but as they are they only confuse instead of shedding light on the subject of counter-irritation in chest

affections. But is it the fact that when a blister is applied to the skin covering a "chronically diseased joint" inflammation is thereby set up in the skin, cellular tissue, periosteum, and bones? Professor Williams not only says so, but he further adds in "this manner counter-irritants are beneficial in chronic inflammations."

Would Professor Williams not go the length of saying that counter-irritants applied to the skin covering a *healthy joint* set up an inflammation in the skin, cellular tissue, periosteum, and bones? If he says no, and that their action does not extend deeper than the subcutaneous tissue, then I ask why in the one case and not in the other? Speaking for myself, I have seen on *post-mortem* the same changes produced by cantharidine blister on a healthy joint as on a "chronically diseased one," and I therefore go the length of saying that in neither case does the local inflammation extend to the bone. If it did it would aggravate the disease, and counter-irritation would do harm instead of good.

But, like Professor Williams, I have also put this matter to a practical test, and that within the last eight days; and I will now briefly detail the results.

No. 1. *Post-mortem* examination of the chest of a horse, which three days prior to slaughter had mustard as a counter-irritant applied:—Inflammatory congestion of skin and subcutaneous cell-tissue, with effusion very pronounced; but periosteum, ribs, and other tissues unaltered in colour or consistency.

No. 2. *Post-mortem* examination of a chronically diseased hock-joint, which had been "piro-punctured," and thereafter blistered, about one week prior to slaughter:—Inflammatory action pronounced on the skin, and markedly at the points of puncture with the "irons" on the superficial layer of subcutaneous tissue; but all structures deeper placed not in the least affected or altered in appearance.

No. 3. *Post-mortem* examination of "chronically" enlarged fore-fetlock joints of a horse, which three days prior to slaughter had been blistered with cantharidine ointment:—Inflammatory action and effusion confined to skin and subcutaneous cell-tissue.

No. 4. *Post-mortem* examination of a "lady-toed" worn harness horse, having a large "splint" or bony growth on the inside of each fore-limb, with considerable thickening of the skin from "brushing," and to which diseased parts cantharides blister had been applied three days prior to slaughter:—Inflammatory action marked on skin and cellular tissue, but no deeper.

No. 5. *Post-mortem* examination of a horse which had a considerable growth of bone, involving the last row of small bones of hock and head of large metatarsal bone on the outside, and which had been "line fired," and thereafter blistered with cantharidine ointment three days prior to slaughter:—Evidences of the effect of the "counter-irritation" well marked, but confined, as in all the previous cases, to the skin and subcutaneous cell-tissue, and leaving the periosteum, bones, and deeper structures to the unaided eye unaffected and unchanged in colour.

When we blister or fire a "chronically diseased joint," I again ask, do we set up acute inflammation in the periosteum, bones, and synovial membranes? If so, how is it that the periosteal and bony growths are, as a rule, not increased in size but reduced thereby, and the mobility not decreased but increased, and the pain not intensified but diminished? Bones expand under the influence of inflammation, and mobility is diminished and pain increased; but such are not the results of counter-irritation when judiciously applied to a "chronically diseased joint."

No remedy allays pain like counter-irritation when the counter-irritant is not actually applied to the inflamed part, but to a part in its vicinity. Explain it as you may, but such is the fact.

Professor Williams admits the benefit to be derived by counter-irritation in chest affections that "hang fire," and in "chronic inflammations of joints." And his explanation in the first instance is "that the gentle stimulation of the skin often induces the beginning of convalescence, and acts as a stimulant to the whole body;" and in the second, that the counter-irritant re-excites "acute inflammation in the skin, subcutaneous tissue, and the bones."

But to my mind there is but one method and one explanation, and that is the allaying of the pain in the diseased part and the restoration of normal circulation and cell growth, but the first must precede the other two. And I maintain that counter-irritants never benefit by re-exciting inflammation in diseased parts (be that inflammation acute or chronic), but by exciting inflammation in the healthy tissue-elements of the skin to which *they are applied*, and thereby concentrating nerve-energy on the locally inflamed part, and by reflex action relieving the pain in the originally diseased part.

In his last contribution to the pages of the *North British Agriculturist*, Professor Williams admits that when he "applied counter-irritants in the treatment of pleurisy or pleuro-pneumonia, the condition of hydrothorax ending fatally was not an uncommon one; but that now this dropsy of the chest is almost unknown, although pleurisy and pleuro-pneumonia are as rife as ever." So that Professor Williams, in other words, charges counter-irritants with inducing a rapid transudation of the water of the blood into the chest; and in support of his contention refers in a delicate way to a "consultant" who ordered mustard to be applied to the sides of horses labouring under influenza, and that "twenty-five per cent. of these animals had hydrothorax the next day, which in every case terminated fatally."

There are always two sides to a question, and I should like to hear what the "consultant" has to say; and until I hear what he has to say I shall offer no positive opinion upon this, to me, most extraordinary result. But I will say this, that I have treated in my day thousands of cases of influenza, and have had probably not fewer than one hundred cases daily on my hands for a month at a time. A large percentage of these animals were subjected to

counter-irritation of the chest by mustard; and the number of deaths due to hydrothorax, pure and uncomplicated, might almost be counted on my fingers; and in not one single instance would I feel justified in saying the counter-irritant brought about that result.

But did Professor Williams examine these horses on the day the "consultant" did? And did he find that there was no liquid detectable in the chest of any one of them prior to the irritant being applied? Did he make a *post-mortem* examination of the bodies of all the animals? And is it the fact that the only chest lesion was hydrothorax? If so, then I should feel greatly obliged by the Professor explaining how the blister produced such "deadly havoc" in so short a period of time, and in otherwise healthy lung-tissue.

Professor Williams seems to be thoroughly satisfied that the application of counter-irritants to the chest-wall induces hydrothorax in a large percentage of cases, and that 25 per cent. of the "consultant's" patients were actually killed by the counter-irritant. He has also charged me "with carefully avoiding answering his proposal that this matter (counter-irritants) be put to the test." But in this he is mistaken, and I take this opportunity of saying that if he be willing, so am I, to treat, say fifty cases of chest disease by "counter-irritation" to the chest and the administration of medicinal agents; and if one of these animals dies from hydrothorax pure and uncomplicated, and which had no existence at and prior to the application of the counter-irritant, I shall pay £50 to the funds of the Veterinary Benevolent Association, he contributing a like sum to the Institution if none of the fifty die from any such disease.

That counter-irritants in the remote past, by the severity with which they were applied, did much harm I freely admit. As an instance of this, I remember when a lad seeing a red-hot shovel applied to the abdomen of a horse labouring under inflammation of the bowels. And I admit that even yet some practitioners and clients consider that the counter-irritant has not had a fair chance until its application has been so severe as to produce more or less sloughing of the tissues to which it has been applied, and as a consequence a permanent blemish. But I fail to perceive the good that can accrue in either case, and am certain that if a moderate amount of irritation will not excite inflammation in the tissue to which it is applied, repeated counter-irritants or powerful counter-irritants will not do so; on the contrary, they will destroy the life of the part and hasten the death of the animal. In practice I apply counter-irritants early in almost every case of chest disease and joint affections. In chest affections I seldom apply the counter-irritant twice, and never thrice, to the same animal. If the counter-irritant produces marked tenderness on palpation and effusion into the subcutaneous tissue I am pleased, and as a rule the animal makes a favorable recovery; on the other hand, if the counter-irritant produces no local action I invariably have a protracted and

imperfect recovery, and not unfrequently a death. But from all I can learn the deaths in patients entrusted to my care are not more numerous than falls to the lot of Professor Williams; and as neither my conscience nor my clients have as yet upbraided me for using counter-irritation in the treatment of disease, I intend to defend and continue the practice.

Professor Williams, on rising to reply, was very warmly received. He said the controversy had, so far, been conducted on the most friendly footing; but in the paper just read the Professor had been rather hard upon him, and he took that to indicate that Professor McCall was now feeling his position rather acutely. Professor McCall spoke about his conscience not accusing him of killing any horses by the use of counter-irritants, but they must observe that Professor McCall prescribes the mildest possible form of counter-irritation. In his first letter to the *North British Agriculturist* Professor McCall entirely agreed that the application of cantharides in chest diseases was very bad practice, and led to very bad results. But in this connection they had to notice that many veterinary surgeons never applied mustard, because it caused great pain and excitement, followed by depression. They therefore held that mustard did more harm than good, and they applied cantharides instead, as being milder. When a horse's breathing was rapid, the application of mustard increased the rapidity of his breathing, Take a horse breathing at the rate of 100 times per minute, and apply mustard to the sides of that horse, and you would increase the rapidity of his respiration till he was no longer able to breathe at all. Blisters undoubtedly caused pain and fever; and an ordinary blister of, say, three ounces of cantharides, applied to the sides of a horse, would, in twelve to fourteen hours, increase the temperature by three degrees, and the pulse by 20 beats per minute. He had a record many years ago of a horse that was examined carefully, and his pulse stood at 50, while his temperature stood at 100 degrees. A blister of three ounces of cantharides was applied to his side, and the following morning his temperature stood at 103, he would eat nothing, and his pulsation was 70 per minute, and his respiration was exceedingly rapid. That blister had an effect which it would have done good to the eyes of Professor McCall to see. The animal was killed after the blister had been on for twenty hours, and what was the result? There was found an inch and a quarter of a yellow exudate in the subcutaneous tissue; and about three-quarters of an inch between the pectoral muscles and ribs the intercostal muscles were inflamed, and the pleural vessels were congested. Take then the case of a horse affected with pleurisy, and his temperature standing at 106 degrees, and heat him with the same amount of blister. By next morning that horse's temperature would be 109 degrees, and a horse would not live long with a temperature of 109 degrees. Therefore, he said, it was running a great risk to do anything to elevate the temperature where such elevation of the temperature

was calculated to endanger the animal's life. Professor McCall spoke about reflex action; but if you irritate and paralyse the vessels of the skin, you paralyse the vessels of the chest and bring about congestion; and while you are increasing the calibre of the superficial vessels that reflex action cause, do you, as stated by Professor McCall, the converse condition within the chest? And does dilatation of the vessels in one place cause contraction of the vessels in another? This reflex action, as stated by Hughes Bennett in his 'Principles and Practice of Medicine,' is still one of the most vexed questions in therapeutics. If you blister one part, not only is the skin and subcutaneous tissue inflamed, but the deeper seated tissues inflamed also, as was evident from the case he had mentioned, where they had the exudate on the lungs. Professor McCall accused him of being dogmatic. Well, perhaps he was very dogmatic because he worked out the facts which brought him to a certain conclusion, and he stuck tenaciously to that conclusion because he firmly believed it to be a correct one. He had lived long enough to see all sorts of treatment applied for chest diseases. In his earlier days he was in Oldham, and there a blister to the breast and a rowel in the breast were regularly prescribed, and the success of that system was great comparatively. In Manchester at that time counter-irritation to the sides was very freely applied, and the success was not to be compared to that resulting from the Oldham practice. When he went to practise in Bradford he was as strong in favour of counter-irritation as he could be, and he lost as many patients as other veterinary surgeons. He had the advantage of hearing Professor Bennett on this subject, who said, thirty years ago, that this was a vexed question in medical science, and it remained a vexed question to this day. Well, he decided to change his tactics, and he did so with the greatest possible advantage to himself. In fact, it was the making of his practice in Bradford, for he succeeded to a ruined practice, and had he not been successful, he would never have built up the practice which he enjoyed. In one single year his practice went up by several hundred pounds, and that was entirely due to his success in the treatment of lung diseases. The challenge to treat 100 cases without counter-irritants, as against the treatment of 100 similar cases with blisters, was not made by him, but was made by the writer in the *North British Agriculturist*, and Professor McCall accepted it. But Professor McCall put the challenge beyond the range of acceptance, for he said that if any of those treated with counter-irritants died of pure hydrothorax, he would give in to the opposite treatment. There was one great thing they must not forget in connection with this application of mustard to the side. Where there was a great deal of exudation there was a proportionately great amount of swelling, and that was the condition they wanted in such a case. They must remember that there was such a thing as a lymphatic stream associated with the body with the pleural surface. These lymphatic vessels radiating from the inflamed surface became surcharged with lymph;

in fact, the little runlets were in spate on the sides of the chest when the pleura was inflamed; and if to this surcharged lymphatic stream you bring an exudation by artificial inflammation, you cause the outpouring of an immense quantity of lymphatic matter in the lungs. In other words, you mechanically block up the lymphatic vessels, and as the lymph is still poured out on the inflamed structure it goes into the cavity of the chest. Professor McCall threw some doubt on his statement about 25 per cent. of some horses treated with counter-irritants having gone down with hydrothorax. He knew it for a fact, however, and he could give the names if need be. The fact, which he was prepared to prove at any time, was explained by his reasoning about the blocking of the lymphatic stream. With regard to his contention that an inflammation when once set up cannot be removed by the excitation of another one, Professor McCall said, look at the acknowledged benefit of blisters in chronic cases. Well, when the inflammation has arrived at a chronic stage, that inflammation certainly was influenced by re-exciting the acute process, but that was precisely what he had all along taught. Professor McCall had said that he (the speaker) stood almost alone in the profession in standing out against the use of blisters in acute diseases of the chest. Well, he felt very proud at having the courage to stand up almost single-handed for what he regarded as a cardinal truth. By not using these blisters he saved the horses all the intense suffering caused by those counter-irritants, and his patients were at work sooner than those treated with these excruciating and weakening blisters. (Applause.)

Mr. Hunting, sen., of South Belton Colliery, said he hoped that Professor Williams and Professor McCall would not go the length of putting themselves against each other as to the greatest number of cures in a certain number of cases, because the conditions under which the patients might be kept might be altogether different. In fact, the gulf between those eminent authorities was not so great as at first appeared. Professor McCall did not approve of the heroic practice of putting, say, three ounces of liquid cantharides and half an ounce of biniodide of mercury on a horse's chest. He (Mr. Hunting) had a good deal of experience of horses, as he had never less than seven or eight thousand horses under his charge, and he agreed with Professor McCall that a mild application of mustard to the side or along the course of the trachea decidedly did, in very many cases, a deal of good. If you could, by stimulating the chest by a mild application of mustard, bring about some of the conditions of the healthy tissues, and, as Professor Williams had said, throw the lymphatic vessels into flood instead of the ordinary action, it was not at all unlikely that you would get a great advantage to the congested tissue, and, however insignificant the benefit might be, the man grossly neglected his duty who did not apply such a remedy. He held that congestion was the first stage of inflammation, and that that was the important point where counter-irritation was exceedingly valuable. On the other hand, everybody who has treated cases of influenza,

or pink-eye, as it is now called, knows that the man who relies upon counter-irritants mainly for the cure of lung congestion or pleuro-pneumonia knows nothing of what the animal wants. What the animal essentially wants in such a case is good nursing, especially good dietetic and hygienic conditions to facilitate recovery. The animal must have pure air, warm clothing, and good nourishing food and pure water. The man who did not blister, but attended most carefully to the essentials of recovery, might be much more successful than the man who made a judicious use of blisters, but failed to attend to the essentials of success, and that fact might to a very considerable extent account for the marked success of Professor Williams in his treatment of this disease. (Applause.)

Professor McCall said that, whenever a case of influenza came into his hands, his first care was to see that the animal was placed under the best dietetic and hygienic conditions, as he knew quite well that these in reality constituted, as Mr. Hunting had said, the essentials of success in the treatment of the disease. But at the same time he decidedly said that the use of counter-irritants had proved a very great help in dealing with the disease. He would not allow this discussion to drop until some benefit had been derived from it, and until they came to a clear and correct understanding upon it. Something had been said about taking the discussion out of the pages of the *North British Agriculturist*, where it had originated, and transferring it to the *Veterinarian;* but he would be only too glad to have the question thrashed out in the *North British Agriculturist*, and only wished the correspondence in that paper reproduced in the journals of the veterinary profession for the benefit of the great body of veterinary practitioners throughout the country. (Applause.)

The *Chairman* (Mr. Hunter) said that, as it was getting late, the discussion would have to be adjourned till their next meeting. They were under a deep debt of obligation to Professor McCall and Professor Williams for coming there to give them the result of their large practice and wide experience on the subject. They could hardly expect these eminent teachers to come such a long way to their adjourned meeting; but if they did, the members of the Northumberland Society would give them a very hearty welcome. (Applause.)

OAT-HAIR CONCRETION.

The above was sent to the Royal Veterinary College by Mr. Green, veterinary surgeon, King Street, Dudley, with the following observations:

"The concretion I forward to you is one of two taken by me from an old Irish sow. I broke it to see what the nu-

cleus was. It was a piece of cinder. The sow could not be got fat; she was very thin. The other one is only two pounds weight. There is no doubt that the formation was started in the harvest-time, when, after carrying the oats, the pigs were turned out on the stubble."

Notes by Professor Axe.—The matter referred to by Mr. Green consisted of six irregular masses of a yellowish-brown substance, having a coarsely laminated structure, and weighing 3 lbs. 11 oz. The surface of the mass had a woolly feel, and it was easily broken down. On being submitted to the microscope, the material of which it was composed was found to consist almost exclusively of oat hairs. This is one of several examples of oat-hair concretion in the pig which from time to time have been brought to my notice.

Pathological Contributions.

SINCE the last issue of our Journal, it appears from the *Gazette* that during the four weeks ended February 14th

PLEURO-PNEUMONIA

has been detected in the following counties in England:—Durham, Hants, Kent, London, Lancaster, Warwick, Wilts, York (West Riding). Also in the following counties in Scotland:—Ayr, Aberdeen, Forfar, Linlithgow, Lanark, Midlothian, Perth, and Renfrew. The Board of Agriculture have slaughtered 61 cattle affected with the disease, and 791 which have been in contact or exposed to infection.

SWINE FEVER

is still very prevalent. Upwards of 200 outbreaks have been reported in four weeks, and 871 appear to have been slaughtered, while a still greater number have died of the disease.

ARSENICAL POISONING OF HORSES.

NINE hunters, the property of Mr. C. Sherwin, of Colbridge, county Clare, who keeps a training establishment, are reported to have been poisoned by arsenic, which by some means was mixed with their food. Whether the result of an accident or not has not been ascertained.

Five of the animals died, and the others are not expected to recover.—*Irish Press.*

THE VETERINARIAN, MARCH 2, 1891.

Ne quid falsi dicere audeat, ne quid veri non audeat.—CICERO.

CONTAGIOUS PLEURO-PNEUMONIA.

IT so often happens that the researches of English investigators are pushed on one side to make room for work done by Continental scientists, that no excuse is needed for reminding our readers that the disease now engaging the attention of our officials and inspecting veterinary officers, viz. contagious pleuro-pneumonia, has been thoroughly worked out, from the standpoint of the morbid anatomist and pathologist, by Dr. Gerald Zeo in 1878,* and by Dr. G. S. Woodhead in 1888,† and it is not out of place to recapitulate the conclusions as to the course of the disease arrived at by them.

1. Irritation of the mucous membrane of the smaller bronchi and of the epithelium of the air-vesicles, directly or indirectly, by some specific virus.

2. Catarrhal proliferation, the result of irritation of the epithelium of the air-vesicles.

3. Passage of the virus from the air-vesicles and smaller bronchi into the surrounding lymphatics.

4. The peribronchial lymphatics are implicated from both of these sources.

5. The block thus caused in the lymphatics of the broncho-vascular system impedes the flow of lymph from other parts.

6. This is followed by the accumulation of fibrinous lymph in the air-vesicles, which soon finds its way into the smaller bronchi, and thence into other air-vesicles, and helps to set up a catarrhal pneumonic process by direct irritation and by the occlusion of the smaller bronchi.

7. The infective material finds its way along the lymphatics to the root of the lung, and so we have certain changes in the blood-vessels and the engorgement of the lymph-spaces.

8. The most varied forms of hæmorrhagic infarctions are thus produced.

9. These infarctions may become gangrenous, putrid, or cheesy, or they may dry up into a crumbling mass.

10. A capsule may form, shutting off the most diseased part from the rest of the lung.

* *Journal of the Royal Agricultural Society*, part i, 1878.

† *Journal of Comparative Pathology and Therapeutics*, 1888.

11. The changes in the pleura are similar to those in other parts of the lymphatic system.

12. The chronic changes are the same as those met with in any chronic interstitial pneumonia, and are brought about in the same manner.

13. It is an infective disease, probably due to the action of an anaërobic micro-organism.

14. The disease has not yet been produced experimentally by inoculation.

And to this very complete summary we may add—

15. The period of incubation has not been accurately determined.

THE RELATION OF DISEASES OF ANIMALS TO THOSE OF MAN.

We learn that a prominent place will be assigned in the discussions of the International Congress of Hygiene, which is to be held in London next August, under the presidency of H.R.H. the Prince of Wales, to the question of how far the diseases of animals are communicable to man, and *vice versâ*. The subject has been very lately much in the air in consequence of Dr. Koch's recent discoveries, and the appointment of a Royal Commission to investigate the subject of tuberculosis. The Organising Committee of the Hygienic Congress have recognised the importance of the question by setting apart a special section for its discussion, and have been successful in associating with the section gentlemen of high authority in the medical, veterinary, and agricultural world. The section proposes to consider, amongst other subjects, the infectious, contagious, parasitic, and other diseases communicable from animals to man, and *vice versâ;* the methods of the propagation of diseases affecting mankind by means of animals and animal products; the infection of meat, milk, and other comestibles; and the restrictions to be placed upon the sale of infected food and the movement of infected animals. On each of these questions papers will be obtained from the highest British and Continental authorities as the basis of the debates of the section, which promise, therefore, to be of very great interest and value. The President of the section will be Sir Nigel Kingscote, K.C.B., Chairman of the Board of Governors of the Royal Veterinary College; and the Vice-Presidents will be Professor G. T. Brown, C.B., Director of the Veterinary

Department of the Board of Agriculture; and Dr. E. Klein, F.R.S., Lecturer on Physiology at St. Bartholomew's Hospital. They will be assisted by a Council containing, amongst other members, Dr. Edward Ballard, F.R.S., of the Medical Department of the Local Government Board; Dr. Edgar Crookshank, Professor of Comparative Pathology and Bacteriology at King's College; Dr. George Fleming, C.B., late Principal of the Army Veterinary Department; Mr. J. T. Reeves, representing the Worshipful Company of Grocers, which has for a considerable period made munificent grants in aid of scientific research; Professor James McCall, F.R.C.V.S., President of the Royal College of Veterinary Surgeons; Professor John McFadyean, M.B., B.Sc., F.R.S.E., Lecturer on Anatomy and Pathological Histology at the Royal Veterinary College, Edinburgh; Shirley F. Murphy, M.R.C.S., Medical Officer of Health for the County of London; Dr. J. Burdon Sanderson, F.R.S., Professor of Physiology at Oxford University; Sir John Thorold, Bart., Chairman of the Veterinary Committee of the Royal Agricultural Society of England; Dr. George Turner, Medical Officer of Health for East Hertfordshire; and Sir Jacob Wilson, one of the Governors of the Royal Veterinary College.

The Secretaries of the section are—Mr. W. Duguid, F.R.C.V.S., Assistant-Inspector of the Veterinary Department of the Board of Agriculture; Dr. G. Sims Woodhead, F.R.S.E., Director of the Research Laboratory of the Conjoint Board of the Royal College of Physicians and Royal College of Surgeons; and Mr. Ernest Clarke, Secretary of the Royal Agricultural Society of England, to the last named of whom communications relating to the section may for the present be addressed, at 12, Hanover Square, London, W.

Extracts from British and Foreign Journals.

SURRA.

"Among the parasites found in the blood of the horse the most remarkable is the monad which is known in India as the surra parasite, the disease with which the creature is associated being termed *surra*, or pernicious anæmia. The affection was at first considered to be analogous to relapsing

fever of man, and the parasite was looked upon as identical with the spirillum which is found in the blood in that disease. Careful microscopic inspection soon demonstrated that the monad of surra is not the least like the spiral thread-like organism the spirillum, but, on the contrary, it has a body somewhat irregular in form, but generally oval, and possesses a flagellum at each end, by the aid of which its violent and rapid movements are performed. No satisfactory evidence of the surra parasite in the production of the disease has been advanced. It is quite certain, however, that the same parasite—or, at least, an identical organism so far as form is considered—is found in the blood of a large proportion of the sewer rats in this country. Very little doubt exists as to the occasional presence in the blood of wandering larval nematode worms. The fluid is not generally considered to be the home of parasites, and the helminthologist rarely thinks of searching for them in the interior of blood-vessels."—*Field.*

BOVINE TUBERCULOSIS IN NEW SOUTH WALES.

"The Board of Health report to the Legislative Assembly of New South Wales on bovine tuberculosis in the South-east District points out that the 'coast cough,' so frequently referred to as being of no very great importance, is really a symptom of tuberculosis. Mr. Edward Stanley, G.V.S., found no fewer than twenty-five tuberculous animals amongst 766 cows in eighteen dairies. As the examination was necessarily somewhat hurriedly performed it is probable that this number would be very much exceeded had a more careful examination been possible. He also found that pigs fed with the milk from certain of these dairies suffered from panting and fœtid ulcerous sores, both of which appeared to be due to tuberculosis. Drs. Gibson, MacLaurin, and Wilson, reporting on six animals that were slaughtered—four cows and two swine—state that these were suffering from tubercle, as in most cases tubercle bacilli were found. Mr. Stanley suggests that the Dairies Supervision Act, Clause 10, be enforced, and that 'the supply of milk which shall have been produced from any diseased animal' shall be cut off, and that 'it is the duty of all local authorities to use their influence to exterminate the disease, which can only be done by destroying the animals, both flesh and milk being unfit for food either for man, pigs, or poultry.'"—*Brit. Med. Journ.*

TEXAS FEVER.

By Paul Paquin, M.D., M.V., State Veterinarian, Columbia.

"Office of Paul Paquin, M.D., M.V.,
"State Veterinarian,
"Columbia, Mo.; *November 8th*, 1890.

"To Editor of the *Journal of Comparative Medicine and Veterinary Archives.*

"Dear Sir,—In the last portion of my bulletin on Texas Fever, published in August number of the *Journal of Comparative Medicine and Veterinary Archives*, Fig. 1 has been so contracted, as I wrote you before, as to be absolutely useless; and then the indication under it is made to read as follows: 'Fig. 1 × 2000,' instead of 'Fig. 1 × 1000,' as it should. Please kindly correct the error.*

"Now, I beg leave to say a few words more in regard to the criticism of Dr. Clement in our investigation of Texas fever, and the discussion and paper on the same subject by Dr. Salmon. I did not wish to tire the audience by further discussion at Chicago.

"I protest against the positiveness and exclusiveness with which these gentlemen explain their own opinions, and the uncalled-for allusion to jealousy which was hurled from such a high official position as Dr. Salmon's; I protest against the evident endeavour of Dr. Clement to cast a reflection on our honesty in our endeavours to fulfil our mission and duty by our people and country in investigation of Texas fever. Dr. Clement purposely or ignorantly selected the first and necessarily crude part of the first year of our investigations, and compares it with the latest labours of the Bureau of Animal Industry, which had been investigating the problem for years, and tries to create the impression abroad that our work is unworthy of notice, whilst he and the gentlemen of the Bureau are impregnable. We know well the abilities of Drs. Smith, Salmon, and others, and none dispute them; we give them credit and due respect. There was no occasion to write opinions in the guise of report and discuss them in such a way as to attack the professional and manly integrity of others.

"Our bulletin No. 11 on Texas fever was a first report (on progress) for our people, as demanded by law, and it is explained therein that it was written for the people generally, and not for scientists. We explain further that as to the germ, we do not mean to describe the biological points, and that we mean later to write on that question in particular

* See next page.

after more complete study. We *do not* classify the germ of Texas fever, as Dr. Clement insists; we used the words bacteria, germs, and so forth, indiscriminately, thereby reaching better the average intelligence. It was a farmers' bulletin, in which the amiable critic could have found some practical points worthy even of his intelligence and consider-

CORRECTION.—THE GERM OF TEXAS FEVER.
(Magnified 1000 diameters.)

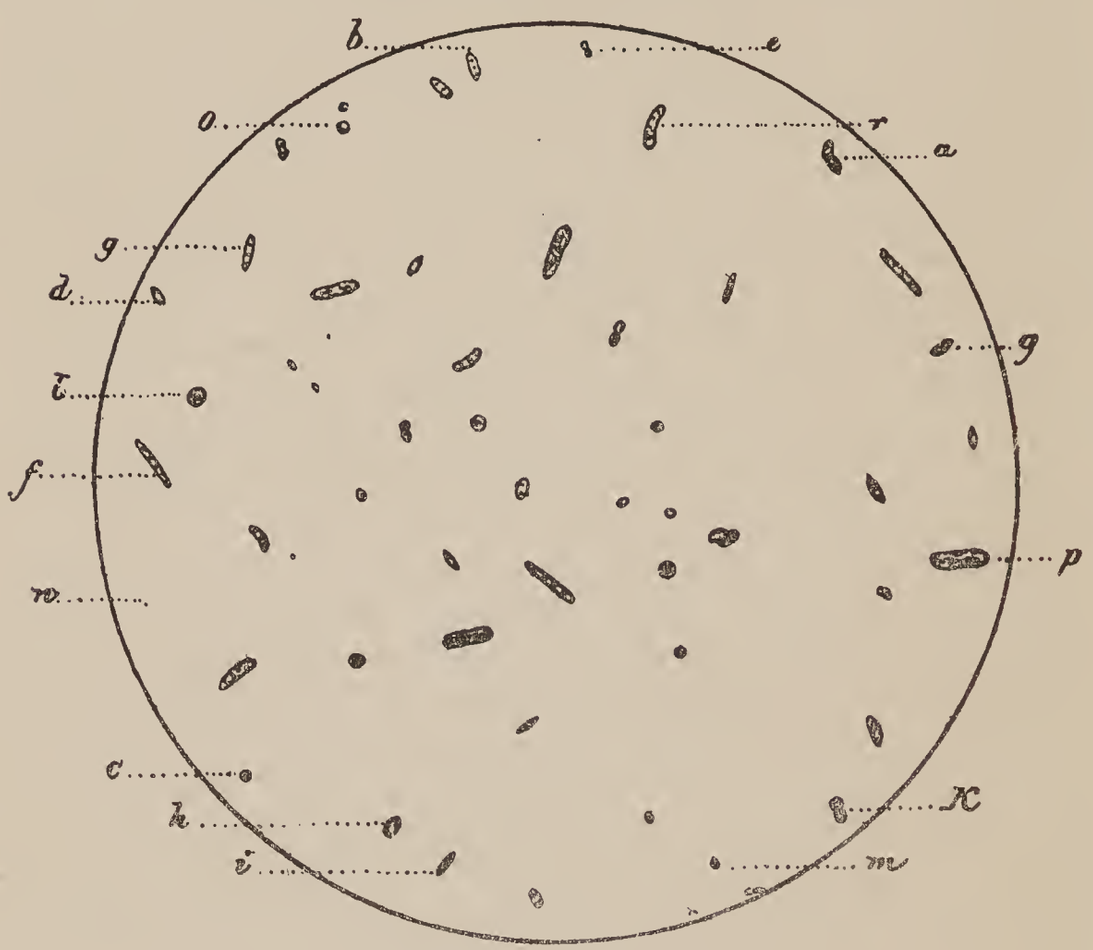

Fig. 1. × 1000.

Explanation of the Figure.—The microbe of Texas fever at various stages of its development, from a shaken liver-broth culture pure, one day old (that became inoculable). Origin from the blood of a native suffering from Texas fever.

Letters *a*, *d*, *h*, point to the *small ovoid stage* described by Frank S. Billings as the adult form; the letters *b*, *i*, *g*, present ovoid forms elongating and spots within; the letters *o*, *c*, as well as the *b*, pointing to a small circle to the left of the figure show the cocoid appearance, or perhaps the same forms as the preceding standing on end, and thus presenting a view of the top of one extremity; *m*, fine new forms to develop in the ovoid represented by *a*, *b*, *h*; *f*, *r*, germs sufficiently developed to show by dots what seems at this stage to be the ends of new ovoid forms within; *p*, still further development of a mother-cell, showing two fine ovoid bodies within, which are to be freed, and appear then as shown by letters *m*, and later *a*, *b*, &c. &c.; *e*, *k*, figure 8 germ—an appearance probably indentical with Dr. Salmon's bacteria of Texas fever.

ation, had he been willing to act as broad-minded as the true man of science is always.

"Dr. Salmon tells us that *tics* carry Texas fever to the North. We had explained that in our bulletin, but Dr. Clement and he ignored it, as they did every other new point of practical value found in our bulletin. The differences in the period of incubation South and North, the means of transmission of the disease, the duration of the period of danger in Southern cattle, the question of immunity, the question of inoculation, were either ignored or treated with such contumely as to cause general comment among those who comprised the audience. Again, Dr. Salmon alludes to our inoculation as being unreliable, because, he would have the people understand, we probably inoculated various germs and obtained a variety of diseases and could not distinguish them. There was no ground for a charge implying, as this does, ignorance of the very foundation of practical bacteriology.

"Dr. Clement (who acknowledged himself ignorant of the clinical history of Texas fever) and Dr. Salmon conclude, from microscopical studies, that the Texas fever parasite and the parasite of malaria in man, as described by Leveran, are identical. Well, suppose it were so, is that sufficient excuse for these gentlemen to try and create the impression that all other investigations in that field were made by men unworthy of respect, and that their labours were not according to truth and good methods? Even if we are found in error in certain points of our investigations and conclusions, do not negative results and dire failures even prove frequently of as much, and sometimes of more value than affirmative results? But it is by no means proven, though very probable, that even Leveran is absolutely right in his conclusion that his micro-organism is *not* a bacteria, and therefore it is endeavouring to make a fact of an assumption to say that Texas fever is *surely* not bacterial, and is identical with malaria. For years the most learned authorities thought actinomycosis was produced by a fungus, and now come Baumgarten, Bostrom, Podwissowsky, Affanassiew, Cornil, Babes, who, after deep studies, classify it among bacteria. Shall we reflect on the knowledge, honesty, integrity of Peroncito, Bollinger, Harz, Ponfic, Israël, and all other scientific men who classified it among fungi?

"Now, to whatever class the micro-organism of Texas fever may belong, it is not yet clear by any means—Dr. Salmon's, Dr. Smith's, and Dr. Clement's opinions notwithstanding—that it is or is not of a bacterial order. In my judgment it

is more rational in this stage of our knowledge to compare Texas fever and its germ to 'hæmoglobinuria of cattle,' described by Babes in Roumania. The blood-corpuscles show about identical lesions, and Dr. Salmon's own lantern projections exposed blood-corpuscles with lesions much like the illustrations of Babes. Here is what this author says:

"'Bacterial hæmoglobinuria of cattle . . . characterised by the condition of urine, *coloured red* or *black* by *hæmoglobin*, . . . and by the seat of special microbes in the *interior* of red blood-corpuscles.' This corresponds with the lesions of Texas fever. Babes is not exclusive, however, and does not pretend, even after long and close investigations, to say positively that the germ belongs to this or that class. He has *cultivated* it with difficulty; he inoculated it and reproduced the lesions in the blood of *small animals* and *cattle* as we had done Texas fever, and have *repeated* since the Chicago veterinary meeting. Babes further states that the microbe 'appears under *various* forms.' Will Dr. Clement ridicule this great author, as he tried to do with us, because we describe various forms of germs in connection with Texas fever? It is an elementary question in biology, the changeable sizes and shapes of microbes in evolution. And why not several different germs? In typhoid fever there has been a variety of germs found which are called now typical, constant, and pathogenic, and capable of causing this fever. This is not probable in Texas fever, but various forms may be present.

"In science none can gain confidence by attempting reflections on others, no matter how humble their station, their work, or their knowledge."

HOG-CHOLERA.

"Selander (*Annales de l'Institut Pasteur*, p. 545, No. 9, T. 4, September 25th, 1890) continues his researches, commenced two years ago, on the infectious diseases of pigs, known under the names of hog-cholera, swine pest, and infectious pneumo-enteritis. He considers that the above are synonyms for one and the same disease, and he describes the symptoms as those of a kind of hæmorrhagic septicæmia. The organism, which he previously described, remains alive for several months in a 7·5 per cent. of salt solution. It loses its virulence when successive generations are cultivated in gelatine, but after being passed through a series of

rabbits it regains its infective power and becomes fatal to those animals. Similarly, when passed through a series of pigeons, it becomes virulent enough to cause their death, though, as is well known, they are much more resistent than rabbits. The earlier cultivations kill pigeons slowly, the later ones rapidly; and they are then fatal when injected into pigs in the subcutaneous tissue, and very fatal when introduced into a vein. When pigs are fed on the organs of rabbits that have died from the disease they usually die, and on *post-mortem* examination there is found marked ulceration of the intestine with swelling of the mesenteric glands. If the organism is subjected to a temperature of 53° C. for twenty-five minutes, to 54° C. for fifteen minutes, to 55° C. for six minutes, to 56° C. for three minutes, it will no longer grow on gelatine; but that the bacteria still remain alive is evident from the fact that they are still mortal to rabbits. They are killed, however, at a temperature of 54° C. kept up for forty minutes, and 57° C. is rapidly fatal. Death, Selander considers, is due to poisoning, as when filtered blood from an affected animal is injected into the vein of a healthy but susceptible animal, the animal dies rapidly, with symptoms of paralysis, which occur in a regular and definite order. When an animal recovers, the poison is apparently excreted by the kidney. The poison is not destroyed at 58° C., but it loses its power at 100° C., and retains only part of it at 60° C.; it is not an alkaloid, but a toxic albumen which has a cumulative action. In some experiments on protective inoculation, Selander found that if he heated a virulent fluid for one hour at 57° C., it protected against the action of virulent blood-cultures of the organism, and, in those cases in which he failed in absolutely protecting the animal, the duration of the course of the disease was often considerably increased. A dose of the actual poison injected into the veins was still lethal, although the bacteria could no longer produce the disease. He never succeeded in rendering either pigeons or pigs refractory. The organism is always more virulent when grown in living animals than as a saprophyte, even when growing on blood-serum; but in the saprophytic stage, although not so deadly to animals, it resists a higher temperature than it does in the parasitic phase."—*British Medical Journal.*

THE PRODUCTION OF IMMUNITY WITH THE CHEMICAL SUBSTANCES FORMED DURING THE GROWTH OF THE BACILLUS OF HOG-CHOLERA.*

By E. A. v. Schweinitz, Ph.D., Chemical Laboratory, Bureau of Animal Industry, Department of Agriculture, Washington, D. C.

"As a continuation of the preliminary paper on the ptomaïnes from the hog-cholera germ, presented by us to the Chemical Section of the American Association for the Advancement of Science, in August, and published in the *Medical News,* September 6th, 1890, we present now a somewhat detailed account of the successful experiments in the production of immunity in guinea-pigs which have been made up to date. The work from this standpoint again is of course a practical continuation of the experiments of Drs. Salmon and Smith, made upon pigeons in 1887, in which sterilized culture-media were used for preventive inoculation. We refer further to the bulletin of the Bureau on 'Hog-cholera,' published in 1889, in which are recorded a number of experiments upon hogs, sterilized culture-media being used for the purpose of producing immunity.

"This work of Drs. Salmon and Smith was the pioneer work in preventive inoculation with other than some form of the germ of the disease itself, and the work now recorded was of course under the advice and direction of Dr. Salmon as head of the Bureau of Animal Industry. Without the careful bacteriological study of hog-cholera which has been made by the Bureau of Animal Industry, our work would have been impossible. For our laboratory experiments guinea-pigs were used, as being convenient to handle and susceptible to hog-cholera. They have proved very satisfactory. The material used for inoculation was prepared in the chemical laboratory by modifications of methods already described, and by other methods which will be explained in more detail at some future date. The testing of the materials used to determine that they were free from germs, and the greater part of the preventive inoculations, were made by Dr. Moore, with such quantities of substance and at such times as we thought best. The autopsies were also made by Dr. Moore, and the work thereby greatly facilitated.

"As to the name which should be given to the ptomaïnes and albumins from the hog-cholera culture-liquids until their chemical constitution is more thoroughly studied, it

* Reprinted from the *Medical News,* October 4th, 1890.

would seem best, as there are several distinct swine-diseases, to call the ptomaïnes from the hog-cholera germs, as a class, *Sucholotoxins*, and the ptomaïne which appears to be the principal factor *Sucholotoxin* (from the Greek Σῦς, a hog; Χολέρα, cholera, from Χολή, bile; and *toxikon*, poison). *Sucholo-albumin* would seem to be sufficiently distinctive for the albumin of these culture-liquids. As Hankin* shows, the name *toxalbumin* is hardly the correct one to apply to these substances. We shall, therefore, refer to the ptomaïnes and albumin by the names given above.

"The first of our experiments that we will record were made with sucholotoxin.

"*Experiments I.*—Two guinea-pigs, each weighing about three fourths of a pound, were treated with a solution of about 0·05 gramme of sucholotoxin each. The solution was introduced under the skin of the inner side of the left thigh. Immediately after the operation the animal appeared uncomfortable, but was not made ill. For a few days there was a rise in temperature and also a slight swelling at the point of inoculation, which, however, disappeared in about five days, and the animal was then well.

"Two more guinea-pigs were now selected as checks, approximately of the same size and weight as those which had been treated, and the four animals were then inoculated with 0·1 c.c. of hog-cholera virus each (0·1 c.c. beef-infusion and peptone culture one day old, *plus* 0·2 of sterile, normal salt solution). This is the dose which previous experiments made in the Bureau had shown to be the proper quantity to kill a guinea-pig in from eight to ten days. The inoculations with the virus were also made subcutaneously in the thigh. The checks died in eight or nine days. *Post-mortem* examination showed a large swelling at the point of inoculation, infiltration of a purulent greyish substance into the connective tissue, and necrosis of the superficial layer of the muscles of the thigh; enlargement and reddening of inguinal glands; Peyer's patches enlarged and pigmented; liver pale and covered with a number of necrotic foci; spleen very much enlarged, dark-coloured, and friable. Cover-glass preparations from the spleen and liver showed hog-cholera germs. This was the characteristic appearance of all the check guinea-pigs upon *post-mortem* examination, and it will not be necessary to repeat these details.

"Of the animals which had been first treated with the substance mentioned and afterward inoculated, one died two days after the last check. Autopsy revealed the following:

* *British Medical Journal*, 1889, p. 810.

At the point of inoculation in the left thigh the subcutaneous tissue was infiltrated with a greyish-white substance, and the superficial layer of muscles over the inner side of thigh and four square centimetres of the abdominal wall were necrosed. Liver pale. Spleen much enlarged, dark-coloured, and friable. Cover-glass preparations from the spleen showed a large number of hog-cholera germs. Both ventricular walls of the heart were light greyish and very brittle (necrosed). The other guinea-pig of this set was quite ill for ten days, with a large swelling at the point of inoculation. This finally opened and healed, and the animal was quite well within three weeks after the inoculation, and has continued so to date—five months.

"*Experiments II.*—The next series of experiments were made with sucholo-albumin from beef-infusion and peptone culture-media.

"Two guinea-pigs were again selected and treated with about 0·008 gramme each of sucholo-albumin. There was a slight rise of temperature in the animals, and the formation of a small, hard lump at the point of injection. This disappeared by the eighth day and the animals were quite well. Two more guinea-pigs were now taken as checks, and all our animals were inoculated with 0·10 c.c. of hog-cholera culture. The checks died within seven days. The *post-mortem* appearances were practically the same as those noted in the first series. The two guinea-pigs which had been treated with the sucholo-albumin died *ten* days after the checks. This indicates considerable resistance to the disease. Autopsy showed, at the point of injection with the albumin, the subcutaneous tissue thick and reddened. The animals were considerably emaciated. At the point of inoculation a cyst the size of a walnut, and composed of a greyish purulent substance, was also found. The muscular wall surrounding this was sprinkled with punctiform hæmorrhages. Peyer's patches swollen and pigmented; mucous membrane of small intestines covered with a dry, yellowish, firm layer of mucus; stomach contained a considerable quantity of liquid; liver pale, and showed fatty degeneration; spleen slightly enlarged and dark. Cover-glass preparations showed no germs, but a culture made from the spleen showed hog-cholera germs. Beneath the peritoneum in the region of the spinal column and in the mesentery was a considerable number of small greyish tubercles. Several other experiments were made by treating guinea-pigs with albumin in varying quantities, all showing resistance, and subsequently immunity.

"*Experiments III.*—Three guinea-pigs were treated with sucholo-albumin, 0·1 gramme being given to each subcutaneously in the thigh. The albumin for two of the animals was derived from cultures containing blood-serum, the albumose given to the third was from ordinary beef-infusion peptone culture. Ugly ulcers formed at the point of inoculation, which healed, however, in from ten to fourteen days, and the animals with the exception of a slight rise of temperature were well.

"Two checks were again selected, and the five animals were inoculated with 0·1 c.c. hog-cholera virus. The checks died respectively in eight and ten days from hog-cholera. The animals which had received the preventive treatment were slightly ill for a few days with swelling at the point of inoculation, which finally opened and then healed nicely, and within a week the guinea-pigs were well.

"Three weeks after the inoculation, one of these animals was chloroformed and examined *post mortem*. Not the slightest scar could be discovered, all the organs appeared perfectly normal, and no germs were found.

"*Experiments IV.*—Four guinea-pigs were treated, two with a mixture of sucholotoxins, two with sucholotoxin and albumin. The injections were made as before, subcutaneously in the thighs, and at intervals extending over a period of four weeks. The sore caused by each injection was allowed to heal before the next one was made. After the animals had recovered from the last treatment two checks were selected, and the six were each inoculated with $\frac{1}{10}$ c.c. hog-cholera virus. The checks died, one in eight and the other in ten days, the *post-mortem* examination showing characteristic hog-cholera lesions. The animals having the preventive treament were ill for about four days, those that received only the sucholotoxins being more dull than the others. There was also slight swelling at the point of inoculation with the germ, which subsided in ten days, after which the animals were perfectly well, and have remained so—four months.

"*Experiments V.*—Six guinea-pigs were inoculated for this experiment, two with solution of the sucholotoxin and four with a solution of the mixed sucholotoxins. The sucholotoxin solution produced only slight local lesions, while the mixed toxins caused ulceration at the point of injection which did not heal for two weeks. The treatment in this case again extended over a period of from three to four weeks. The animals having by this time recovered, the test-experiment with hog-cholera virus was tried. Four of the ani-

mals mentioned above were taken—two from each set—and also two checks, and the six were inoculated. The checks died in eight and nine days, the autopsies showing the characteristic conditions of death from hog-cholera. Those that had the preventive treatment were ill and dull for from four to six days after the inoculation. At the point of inoculation there was also some swelling and infiltration, very slight, however, compared with the similar swelling on the checks. In the treated animals the swelling sloughed and healed, and within ten days after the inoculation they were perfectly well. To test the resistance of the animals that had been treated by this method, to ordinary exposure the following experiments were conducted.

"*Experiments VI.*—Two guinea-pigs that had received the preventive treatment, two blanks—*i. e.* animals that had received no treatment—and two check animals that were inoculated with hog-cholera virus were placed in one large cage. The checks became ill and died in eight or nine days from hog-cholera. During this time the cage was cleaned only three times, so as to give full and free opportunity for contagion. One week after the checks had died one of the blanks became ill, and died within ten days. The autopsy showed hog-cholera lesions. The second blank became ill a few days after the first blank succumbed, and died within thirty days. The animals which had the preventive treatment are now and have been quite well, though continually exposed for five weeks to every opportunity for contagion.

"*Experiments VII.*—These experiments are a step in advance of those already recorded, and although not quite so conclusive, indicate that the proper methods have been adopted.

"A pure chemical compound, prepared synthetically in the laboratory, was used for treating the guinea-pigs. Three animals were taken, and this compound was administered to them by the method already used. There was a slight rise in temperature of the animals, and swelling and soreness at the point of injection. After this had healed these animals and two checks were inoculated with $\frac{1}{10}$ c.c. of hog-cholera culture. The checks died in eight and nine days. The animals which had been previously treated became ill, two dying five and six days after the checks. The third entirely recovered.

"*Post-mortem* examination of the two that died showed the following:—At the point of inoculation the skin had sloughed away over an area of 1 sq. cm. The superficial

muscular layer was necrosed over an area of about 3 sq. cm. and to a depth of 1 mm., lymphatics in the fold of the knee much enlarged; Peyer's patches enlarged and pigmented; spleen *very slightly enlarged and not discoloured;* kidneys reddened; lungs normal. Cover-glass preparation from the spleen showed a few hog-cholera germs. On both sides of the spinal column were several greyish tubercles, from $\frac{1}{4}$ to 2 mm. in diameter, lying just beneath the peritoneum. This material is being more fully tested, and experiments which promise to be successful are also being made upon hogs. Autopsies made from the animals of Experiments VI., three or four weeks after their recovery, showed that the parts were perfectly normal, not even a scar being left upon the skin, and the immunity produced was therefore *perfect*.

"It is important to add that in all the experiments great care was taken that the solutions used were free from germs, cultures always being made. In cases in which the albumin is used this is particularly important. A single precipitation with absolute alcohol does not suffice to destroy the germs, and it is necessary to free the solution from germs by means of a Pasteur filter, or in some other suitable way. Therefore experiments made with material which has not been tested for germs are practically of no value. As to the poisonous character of the ptomaïnes, a single large dose is sufficient to kill a guinea-pig in from one hour to two days. The autopsy of a case of this kind is as follows:—Liver pale and fatty; subcutaneous tissue over abdomen necrosed and infiltrated; muscle soft and friable. Other organs apparently normal.

"The experiments here recorded show—

"1. That in guinea-pigs *complete immunity* from hog-cholera can be produced by *chemical inoculation*.

"2. The sucholotoxins and sucholo-albumin are equally effective in this respect, and a mixture of these two products gives greater immunity than either used by itself. The effect of the albumin in producing immunity from anthrax has already been pointed out by Hankin, his experiments being very successful.

"3. The sucholotoxins given in large doses produce death. To produce immunity they should be administered in small quantities at a time and at frequent intervals, the system being in this way accustomed to the poison and enabled to resist it.

"Further study in this interesting line of work is in progress.

"The tabulated results of the foregoing experiments are appended.

"TABULATED RESULTS OF EXPERIMENTS IN PRODUCING IMMUNITY FROM HOG-CHOLERA IN GUINEA-PIGS.

Number of experiment.	Material used for treatment.	Hog-cholera virus used for each animal	Number of animals used.	Number of checks.	Number of days between inoculation with virus and death of checks.	Result in treated animals.
I	Sucholotoxin. .	0·10 c.c.	2	2	8 and 9	One died in 11 days; 1 recovered.
II	Sucholo-albumin	,,	2	2	7	Died in 17 days; great resistance.
III	Sucholo-albumin	,,	3	2	8 and 10	Recovered; immunity.
IV	1. Sucholotoxins .	,,	2	...	...	Recovered; immunity.
	2. Sucholotoxin and albumin	,,	2	2	8 and 10	...
V	1. Sucholotoxin. .	,,	2	...	...	...
	2. Sucholotoxins .	,,	2	2	8 and 9	Recovered; immunity.
VI	Sucholotoxins .	,,	2	2 blanks, 2 checks	8 and 9	Blanks died in 18 and 30 days. Others not affected; immunity.
VII	Pure chemical .	,,	3	2	8 and 9	Two died in 13 and 14 days. Third recovered; immunity."

IMMUNITY OF FOWLS AGAINST ANTHRAX.

"Dr. K. E. Wagner (*Annales de l'Institut Pasteur*, p. 570, No. 9, t. 4, September 25th, 1890) has repeated Pasteur's original experiments of producing anthrax in fowls by placing them in water and so lowering their temperature, and he has made a number of observations to determine, if possible, how this lowering of the temperature acts. Sutures saturated with spores of the anthrax bacillus were passed into the anterior chamber of the eye of a fowl, and also subcutaneously. On withdrawing the threads at the end of twenty-four hours he found that numerous bacilli had developed, and a few leucocytes had invaded the thread; that at the end of forty-eight hours there were fewer bacilli but many leucocytes, a few of which had taken in bacilli; at the end of seventy-two hours there were few free bacilli, but many leucocytes and many phagocyte cells containing degenerating bacilli. He concludes that where leucocytes can get access to the bacilli the latter do not retain their virulence for long. An additional proof of this he finds in the fact that when anthrax virulent to rabbits is injected into the circulation of the fowl it is gradually destroyed much more rapidly than in the subcutaneous tissues or in the anterior chamber. The anthrax bacillus outside the body is gradually attenuated at from 42° C. to 43° C., and is killed in nine days; it does not form spores at this temperature. Although fowls do not usually contract anthrax, the injection of bacilli almost invariably produces a rise in their temperature of from 1° to 2·5° C.; in one case the temperature rose to 44·3° C. The fever subsides as soon as the bacillus disappears from the blood. The poison appears to act on the nervous centres, giving rise to altered temperature, but the destruction of the bacilli is due, Wagner thinks, to the activity of the leucocytes, which display the most vigorous movements at a temperature of 45° C. to 46° C. He succeeded in producing anthrax in fowls, to which he had administered antipyrin in order to reduce their temperature. Chloral hydrate also paralyses the leucocytes and allows of the production of anthrax, but not with any degree of certainty. He concludes that the temperature acts not directly on the bacillus, but indirectly by increasing the activity of the phagocytic action of the leucocytes; that the bacillus may still preserve its virulence in the blood of the fowl, although it does not produce any symptoms beyond rise in temperature; that where the

leucocytes are altered by cold or anæsthetics, &c., they cannot readily get to the points of infection, and the bacilli multiply rapidly, as they are no longer destroyed, and may eventually give rise to charbon in the fowl."—*Brit. Med. Journ.*

NATIONAL VETERINARY ASSOCIATION.

THE eighth Special General Meeting of the Association was held October 2nd, 1890, at the Royal College of Veterinary Surgeons—Professor Williams in the chair.

"SOUNDNESS AND UNSOUNDNESS IN HORSES."

An elaborate paper on this important subject, embodying the opinions of many of the leading members of the profession, was read by Mr. G. A. Banham, F.R.C.V.S., Hon Secretary.

The Discussion.

Professor McCall, in opening the discussion, said: Mr. President and Gentlemen,—One of the duties, if not the principal duty of this meeting, is, I understand, to receive and to discuss the paper which has been put into my hands written by our friend Mr. Banham, on the subject of soundness and unsoundness in horses. When I received this paper, having previously consented to open the discussion on it, and had glanced over it, I thought I had undertaken a duty which was impossible for me to discharge, and I am not prepared to say that I am able to discharge it; but I am prepared at all events to acknowledge that Mr. Banham, in the manner in which he has worked out the paper, had a far better idea of how the subject should be treated than I had conceived. I observe that most of you—in fact, all of you—will have observed that Mr. Banham has entirely effaced himself in this production, and has put before us the opinions of several gentlemen who have studied the subject, and whom we may look upon as authorities. Now, I think in doing this he has done wisely and well. I heard some gentleman yesterday make the remark that he expected that Mr. Banham would have written his paper entirely upon his own authority, and given only his own opinion. Now, I hold that an individual opinion upon a subject of this kind is really of very little worth; and I hope those present will take the same view as I have, that, in giving us the opinions of different gentlemen who have studied the subject, he has put before us a paper of much more value than if it was one emanating from himself. With these few remarks, then, I shall refer to some of the points that I consider of the greatest importance in this paper. First of all, under the heading "Question I. The term 'Sound,'" we have the opinion stated

of Professor Axe that no horse should be certified as sound, but that all certificates should signify that the animal is sound so far as we can detect; the wording of the certificate should read, "I do not detect any unsoundness in him or her." Then we have the opinion of Mr. Olver on this subject. He says, "Our clients would not accept that form of certificate, and the public would have to be educated up to it before they would." I have looked over the paper, and have selected for my observations the opinions of those that I thought were more in keeping with my own. Upon this same subject Mr. Stanley writes that "veterinary surgeons giving such certificates as Professor Axe suggested would leave the impression on the public that they were not confident in their own opinions." Then we have Professor Axe saying that "if he appeared not to be confident he would be discreet. He thought that a certificate should refer to soundness negatively, and not positively; and he proposed the certificate should read thus: I certify that I have this day examined a bay gelding_____years old for_____ _____, and I do not detect any unsoundness in him. This was put to the meeting, and all voted for this form of certificate except one member." It must be very evident to all of you that if we did take this form of certificate we should be at all events adopting a form of certificate which was new, and a form of certificate with which our clients are not conversant, and a form of certificate which I have no hesitation in saying they would not look upon as bearing the same authority as the certificate we are now in the habit of granting. Why should we alter our certificates or adopt this negative form of expressing our views of the soundness of an animal? Professor Axe writes, "I do not detect any unsoundness." I can adopt the same form of certificate as Professor Axe here adopts, and it would read in this way: "I certify that I have this day examined the bay gelding_____years old, and I am of opinion that the said horse is sound." I have no hesitation in saying that the form of certificate that I should grant would be much more acceptable to the client than the form of certificate that Professor Axe now suggests. The point for us is this: is there any advantage to be derived by adopting the form of certificate proposed by Professor Axe? Over and above what I have stated, suppose that we both examine the same horse. Professor Axe says he does not detect any unsoundness in the horse. I say I am of opinion that the horse is sound. We both overlook the fact that this horse is a "shiverer." Our clients come to us. They say to me, "You certified that this horse in your opinion was sound." They go to Professor Axe, who says he did not detect any unsoundness. Would our client be in the least better satisfied with the opinion of Professor Axe than with the one I have given? I do not think he would. Suppose, however, we were dragged into a court of law, would Professor Axe's position be better than mine? Certainly at the first glance it would appear that he was in a better position than myself, because he has said, "I certify that I did not detect any unsoundness." Neither did I. I certified that the horse was in

my opinion sound, and so he was. I hold that the result, whatever it might be, favorable or unfavorable, would not be in the least affected by the form of certificate that had been granted. So that practically—in everyday practice and legally, if required to define it in a court of law—either form of certificate will just be as good a one as the other. Why then, I repeat, should we adopt a form of certificate against which undoubtedly our clients will kick, and which undoubtedly will lead to some suspicion that we have a doubt in our own minds in regard to this animal?

I leave this, and I shall now pass on to "Soundness" in Horses, Part I. "I. The public being so conversant with the terms 'Sound' and 'Unsound,' is it advisable to alter them?" We have four, then, saying that it is not advisable, and two gentlemen giving an opinion that it is advisable. Now I do not myself see that there would be any great advantage from altering the terms "Sound" and "Unsound." You might add to them, but I do not think there would be any advantage gained by altering them in any manner or way. I shall therefore leave that point for further discussion.

Then we come to "II. The examination of a horse as to *soundness* leads to an expression of *opinion* being given by the examiner. Should this opinion have any relation to the conformation of the animal? In other words, is it right and proper to refer to conformation when asked an opinion on 'soundness'?" Now here we have seven gentlemen saying, "Yes, it is right and proper to refer to conformation," and we have thirteen gentlemen saying it is not right. Just let us hear what some of them say. Mr. H. Olver says, "Yes;" he says it is right and proper to refer to conformation if such conformation is likely to lead to unsoundness with work. Then we have Mr. Ravenhill: "Unless specifically and specially asked for, and then it may or may not be touched on, as it is considered it might seriously disorganise the positions of the dealer and the profession. A certificate should be plain, &c., any further opinion to be expressed privately and separately." So that he is decidedly against any reference being made to conformation. Then Mr. Duck says, "Most certainly. For instance, in speedy cutting in a saddle horse it is right to inform the purchaser that from conformation and action the horse is liable to, or does speedy cut, which may result in serious consequences to both horse and rider." Then we have Mr. Anderson. We have also Mr. Cunningham, who says, "Nothing to do with conformation as a general rule, but extreme cases may be pointed out." Now, speaking for myself, I may say that I hold a very decided opinion upon this point. I think that conformation ought to be referred to in our certificates. Often do we find in heavy cart-horses that they have got what I call flat feet, flat soles, and weak heels. It does not amount to an unsoundness unless the actual formation is observed. There is no disease present, never has been. I cannot condemn that horse as being unsound, but from the weak condition of his soles and heels, I believe that before long he will be a lame horse, and under these

circumstances I invariably make it a point to refer to the conformation of that horse's soles and heels. I am therefore strongly of opinion that in all cases we ought decidedly to refer to conformation.

We shall now pass on. "Do you express adherence or not to the following paragraphs? — III. A certificate is the written opinion of an individual. All say Yes except No. 10, who does not answer." I think we need not take up any time about that. A certificate is undoubtedly a written opinion; if it is not, I do not know what else it can be. Coming now to No. 4, "A certificate must contain allusion to all defects, so as to be a worthy guide to the client." Mr. Dwyer says upon this, "Not unless such defects infringe upon the question of soundness. Value and capabilities are open to the criticism of the public generally, and do not come especially within the province of the V. S." Now we pass down to No. 9. Mr. Anderson says, "All deformities and defects (not of conformation), even if not likely to interfere with the animal's usefulness, should be brought to the notice of the client." Then passing down to 20, Mr. E. Wallis Hoare says, "Not defects of conformation;" so that we have to point out all defects except defects of conformation. Now my practice is not to point out all defects. If the defects in reality amount to unsoundness, I mention the principal defect, and condemn the horse as being unsound. If we were to mention all the defects I think the certificates frequently would be certificates of extreme length. Undoubtedly if the horse is so defective in some parts that it amounts to unsoundness, it is right that we should direct the attention of our clients to it. With regard to this matter of the certificates containing allusion to all the defects, fourteen gentlemen answer under reservation, and four say "Yes" decidedly, so that those who answer under reservation are much more numerous than those who give a pointed answer.

We will now pass on to No. 5. "A certificate should be the *same* written opinion, and should create a truthful impression in all circumstances under which a horse may be examined as to soundness, viz. (*a*) In the presence of the client; (*b*) Client in reach of personal interview; (*c*) Client in reach of letter only; (*d*) When client has never seen the horse; (*e*) When it is not known from the client for what purpose he wants the horse." Now in regard to this point Mr. Dwyer says, "The certificate proper should be the same under all circumstances. Information tendered to a client under the various conditions, *b*, *c*, *d*, and *e*, should be external to, and not embodied in, the certificate of soundness." Colonel Ravenhill says, "Certainly, but a certificate as above said should only refer to the question of soundness. Any further information sought for, if furnished, should be given in the shape of a letter or verbally." Then we have Mr. Rutherford on the same point. He says, "But if it be not stated by the client for what purpose the horse is required, no defects need be mentioned unless so bad as probably to lead to unsoundness." Well, gentlemen, I think in a

general way we ought always to give the same opinion, it does not matter what our varied circumstances are. As it is here put before us these certificates are our written opinion, and we should endeavour to create a truthful impression under all circumstances. I do not think I need say much more about that.

We now pass to Part VI. Marks of identity. "Marks of identity of the horse should be appended to every certificate (this would allow of the introduction of peculiarities of conformation, and so obviate an accumulation of remarks in the body of a certificate)." Here, again, we have the best opinion. "All say Yes except Nos. 10 and 11, who think it unnecessary." Let us read one or two of the remarks which are made. Mr. Dwyer says, "Marks of identity should certainly be mentioned, but limited to those points necessary for accuracy of description or necessary for identification." Mr. Ravenhill says, "Such natural marks as may be absolutely necessary, together with colour, sex, age, sufficient to identify an animal, should be appended, but no peculiarity of conformation, &c., should be inserted in a certificate." Mr. Cunningham says, "Seldom found necessary." "Unknown" says, "Not necessary." I think under all circumstances it is most important that we should append to our certificates the marks of identification, and it is my practice, and I always enforce it upon my pupils, never to grant a certificate without so doing. I have on several occasions found the utility of so acting. I remember one case that happened a great many years ago, when Professor Pritchard and myself had occasion to give evidence in a court of law in England upon the examination of a horse that had taken place a considerable number of months before. The whole matter had passed out of my mind, and I was sadly exercised about it. I had, however, kept a copy of my erticficate, and I read to them the marks of identification which were all there; and they all said they were extremely obliged to me, as it had refreshed their memory, and brought the whole thing back. Instead of being a question seldom necessary, I hold it to be a thing that should never be departed from.

We now come to No. 7. "That only the word 'sound' should be used to express that condition, to the exclusion of such qualifying words as 'perfectly,' &c." Now, we have remarks here upon this matter which, I think, are pertinent to the question. No. 3: Mr. Stanley says, "The word requires no qualifying adverb." For myself, I think a better answer could not be given. "Perfectly sound:" it is not needful to put "perfectly" there at all; if the horse is sound he cannot be made more sound by using the term "perfectly sound."

We now come to No. 8. "Will you state your objections, if any, to the classification of all horses into four groups, the opinion of the examiner deciding as to which group the horse belongs, the groups being under the following headings and definitions?" Here we have eight gentlemen who had no objection to this classification, and we have five who decidedly do object. Mr. Cunningham upon

this point says, "Too complicated for common use; a horse should either be 'sound' or 'unsound' in law and in everyday business. Practical or specific soundness may, however, be indicated if thought necessary or expedient." Then Mr. Davis, "In writing a certificate it must be 'sound' or 'unsound' if your client is not present, and if he be present the opinion must be decisive, one or other; therefore it would be impossible to classify them." Mr. Hoare also says, "'Sound,' 'practically sound,' and 'unsound' are the terms I use." I have very much pleasure indeed in agreeing with Mr. Hoare. I said I saw no reason for altering the terms "sound" and "unsound," but we might add to them. We have a very useful addition in this "practically sound," but I shall have more to say on this point as we proceed. Under "Sound" we have ten gentlemen who agree with this definition. I shall read the definition: "That condition of a horse in which he is free from disease, injury, or results of injury or surgical treatment, and altered conformation resulting from wear, alone excepting such very slight defects as shall not depreciate him in appearance, in pecuniary value, or in capabilities." It is a long definition. If you will be kind enough to refer to No. 4, Mr. Dwyer, you will see he says, "This definition should, in my opinion, end at the word 'wear,' unless some urgent reason can be assigned for its retention which does not occur to me;" so that, instead of this lengthened definition, Mr. Dwyer would go so far as the word "wear." Mr. Ravenhill writes also much to the same effect. Mr. Rutherford says, "No objection if you strike out all after the word 'wear;' we have nothing to do with appearance, value, or capabilities (say for racing, &c.)." I think he is quite right there. As a fact we are very little asked to give an opinion on the value of the horse, and I always fight shy of ever giving such an opinion. A horse may appear to be worth a considerable sum, and when he is practically tested he may be worth very little indeed. We have nothing to do with the price at which the animal is changing hands; what we have to do is to examine the horse for soundness.

We shall now come to "B:" "Practically Sound." "That condition of a horse in which disease, defects, injuries, or results of injuries or surgical treatment, are such that in the opinion of the examiner they do not prevent, nor are likely to prevent, the horse performing work equivalent to that performed by a sound horse." All agree with this definition except Nos. 3 and 5. Mr. Stanley says, "'B' and 'C' mean 'unsound,' and require the examiner's note of explanation or an expression of his opinion as to the relative importance of the defects." I have not much at the present moment to say against this definition of practically sound, but I may have something to say farther on. I will now refer to "C:" "Unsound, but Horses of utility for Specific Purposes." The definition is, "That condition of a horse in which disease or defects, the results of disease or accident, injuries or the results of injury or surgical treatment, are in the opinion of the examiner of such a nature as to restrict the value of a horse for utility, but not inca-

pacitate his working powers for special purposes." Here we have Mr. Hartley saying, "Responsibility too great, and the group is unnecessary." Mr. Hoare says, "I never give certificates with 'C' class, only a verbal opinion." I am in the habit of doing identically the same thing. I think we are in the habit of dividing it too much. Now we come to "D:" "Unsound." "That condition of a horse in which disease, defects, injuries, or results of injury or surgical treatment, are, in the opinion of the examiner, present to such a degree as to prevent or diminish, or be likely to prevent or diminish, his working powers mechanically by distress or pain." Seven gentlemen agree to this definition, and four do not agree, and others do not mention anything for or against. Mr. Savage says, "Unsound should mean not fit for any kind of work." I do not understand that. There are many unsound horses; probably there are as many unsound horses doing work in the streets of London as there are sound horses.

We now come to No. 9. "That certificates as to soundness given by Fellows and members of the Royal College of Veterinary Surgeons should assume ONE form. As an example for a good form the annexed is proposed.

Date_____

"This is to certify that I have this day examined a bay gelding, the property of (or at the request of) _____at_____. My opinion is—That he is six years old, has a small cicatrix resulting from an incised wound on the inside of the off thigh, and is sound.

"*Marks of Identity.*—Star, narrow race and snip to near side, off hind pastern and heel white." Here we have twelve gentlemen expressing a qualified opinion. We have five adopting this as a good form of certificate. We have Mr. Cunningham expressing an opinion that it is too strait-laced. I quite agree with him. I should not like to be bound up only to one form of certificate. Then we have Mr. Rutherford, who says, "Wording of certificate relative to soundness only to be on face; marks of identity at corner or back. No mention need be made of very slight marks in body of certificate, unless in such a position as to cause the animal to be specially classified, *e. g.* scars on knee or knees, below saddle or withers, &c." Mr. Savage says, "Certificate to be worded 'sound,' or 'practically sound,' or 'unsound for work required.'" Then if you will look to the foot of the page I think you will find a certificate which appears to me to be a very good one: "I hereby certify that I have examined a bay gelding five years old at the request of_____, and am of opinion that said bay gelding is at the present time sound." Now I must say, if I am to express an opinion upon the certificates before me, I may say I should certainly prefer the last. If you will refer to page 36 you will see it stated, "This certificate may be varied after the words 'my opinion is' according to the conditions to be remarked upon, and to terminate with 'practically unsound,' 'unsound but suitable for——,'

or 'unsound.'" Here Mr. Hartley says, "Opinion as to a scar is weak, because patent to anybody. Ought to follow positive opinion in this form: 'My opinion is he is sound. Observe, &c., to which I attach no importance.'"

I shall now ask you to pass on to page 47. I have taken no notice of Part II; I shall leave that for some others of your number who probably are more interested in it than I am. I shall therefore pass Part II over, and refer to just one or two points farther on. On page 47 Mr. Banham shows his own hand. "With regard to the terms 'sound' and 'unsound,' it matters little what terms are used providing we, as professional men, all give them the same definition. (Hear, hear.) To illustrate this we may quote an instance which occurred to us. A gentleman bought a well-formed cart-horse having two small sidebones, and our certificate was to the effect that the animal was 'practically sound.' Our client came to us, and asked what we meant by the term 'practically sound.' We told him that any animal having a defect which as far as we could judge would not interfere with his future usefulness was one which we termed 'practically sound.' 'Well,' he said, 'that is satisfactory, but I asked you this question because a few days ago another veterinary surgeon examined a horse for me and sent a certificate of "practical soundness" for a horse that was dead lame from a sprained tendon, and when I asked him what he meant by the term, his answer was the horse was lame, but not sufficiently to cause it to be unsound, so I called it *practically* sound.'" Well, then, I do not think that that gentleman would have passed his matriculation examination. He seemed not to have understood at all what was meant by the term "practically sound." The horse was going dead lame, having a sprained tendon, and yet he says it is practically sound. It is generally understood by the term "practically sound" that the horse is not going lame, but that there is something on him which may at a future time cause him to go lame, but he is not going lame when we give the certificate; I think that is generally what is meant. I would not like it to go out to the public that we have very many men in our profession who know so little as that about the term "practically sound."

Then we come to No. 2. "With regard to the 'conformation' of an animal being noticed by us in our certificates, we are strongly of the opinion that it should be when it predisposes to disease; and few defects of conformation are free from this drawback." I shall not read further. I am of opinion that we have many defects of conformation which do not predispose the horse to lameness. No doubt there are defects of conformation which do predispose the horse to become lame, and I have very much pleasure indeed in endorsing what Mr. Banham has said. There are several other points of great importance that I should like to have referred to, but it would be wrong on my part and inflicting a burden upon you to listen to me when there are so many gentlemen sitting round this table who are well able to speak on this subject. I have very

much pleasure in expressing my high sense of the paper which Mr. Banham has been kind enough to introduce for our discussion.

Dr. Fleming in continuing the discussion said that Professor McCall had stated his opinion in such a very lucid manner that what remained to be said upon the matter might be put in a few very brief words. He thought there could be no doubt that the word "sound" meant what it implied. The horse was either sound or unsound. He did not admit of the term "practically sound" as one that should be used. He thought it led to a large amount of misapprehension, and that they ought to write the term "sound" or "unsound." Opinions would differ as to what was really practical soundness. He thought, therefore, that the term "practically sound" should not be used. If there was anything at all which was a departure from soundness, except in a professional sense, that should be stated separately. A horse might be perfectly fit for a large amount of severe work and yet be unsound; but yet again a horse might be in the opinion of a veterinary surgeon practically sound, that was, he meant, for certain kinds of work. He thought himself that the clearer the definition was that was given on the certificate the less chance there was for misapprehension afterwards, so that the terms "sound" and "unsound" should be those which ought to be used most. The term "sound" of course was a wide term, because their profession was evidently very much divided as to what soundness really implied. He maintained that it implied a state of health; that was the absence of disease. If a horse had suffered from a disease, and that disease left its traces, then the horse was not sound—could not be said to be sound. That was a very important point. With regard to conformation, he thought they had nothing whatever to do with conformation. Horses were often brought before them with very bad conformation. But provided they were not put to a severe strain on the part which was defaulted they might work for years. They had often found horses of very bad conformation which could do a large amount of work. It was the same thing with men. They often saw men who were badly formed, but who could do a large amount of work. No medical man would pass a man as unsound simply because he had a defect in his conformation. He thought, therefore, it should be kept altogether apart from the certificates. If an opinion was asked, and they pointed out to the owner that his horse was certainly a very good one, but that his conformation might predispose him to disease, all very well and good; but it should be remembered that he might be able to work for years although he was so predisposed, and that should also be told to the owner. If his conformation eventually led to disease, then the horse was unsound; but he thought that the question of conformation should not be recognised by the veterinary surgeon. Then as to soundness for any kind of work, if a horse was passed as sound the question of the kind of work should not be considered; the horse was either sound or unsound. They could

not tell what kind of work the horse was adapted for. Horses with flat feet were no doubt predisposed to disease, but he did not think that unless the disease was present it should be mentioned. A horse with flat feet might be able to work on soft ground very well, but if he was put on a hard street he might fail. If there was no disease at the time he was passed he did not see that it should be mentioned. He did not see that they had anything to do with defective conformation, except sometimes as a favour to the owner in telling him that his horse might not long stand a certain kind of work. As for marks of identity, he thought they were extremely necessary. He thought that in every certificate which a veterinary surgeon gave he ought to be able to identify the animal by the marks he had observed. How else could they recognise an animal again? And not only so, but the certificate was retained by the owner, and without it if he lost the horse he might have a trouble in finding it again. It might be sold to two, or three, or four persons, and only by those marks of identity could that horse be recognised. He also thought that the marks of identity should be as wide as possible, because they would sometimes find horses with the same marks. Face marks were not much guide, because they often found horses with faces with no marks at all. In his experience leg and foot marks were far more valuable than face marks; and they should be of such a kind as to lead, if possible, to the identity of the horse in the future. With regard to the certificates, he entirely agreed with Professor McCall that the last on the list was the best. They ought to be as careful as possible in the wording of their certificate; it should not commit the professional man to anything but what he said in it. It should neither have reference to the past nor to the future, but to the present condition of the horse, as it was impossible that he could see through and examine everything. He ought to make his expression as general as possible. They had not the same facilities for examining animals as doctors had for examining patients. Therefore they should guard themselves as well as they could. He therefore thought that was a very good form of certificate for a veterinary surgeon to use. With regard to soundness or unsoundness, he thought that very often the unsoundness was in the veterinary surgeon's head and not in the animal. It was not the internal organs and things which they could not see and decide upon that should be called the unsoundness, but things which were observable and easily distinguished by the eye and ear, and accessible to the hand. About those there could be no doubt. The question as to soundness or unsoundness was one of expertness, and therefore the veterinary surgeon who examined a horse should be sufficiently skilled and a sufficient expert as to discover from the externals of a horse whether he was unsound or not. He thought Mr. Banham deserved a large amount of credit for the manner in which he had drawn up the paper. He thought he had placed the matter before the meeting in a very satisfactory way. The question of soundness and unsoundness was an everlasting one in the profession and also

in the law courts, and therefore if that paper brought the opinions of veterinary surgeons into harmony upon that matter, the labour which Mr. Banham had bestowed upon it would be extremely beneficial.

Mr. W. B. Walters said he only wanted to say a few words with reference to certificates. He quite agreed with what Professor McCall and Dr Fleming had said with regard to the necessity of giving a definite certificate of soundness or unsoundness, as the case might be. He also quite agreed that the last certificate on the list was the best one they could adopt. As to conformation he was inclined to agree with what Professor McCall had stated as his opinion. He thought that in some cases it was very urgently necessary to mention defective conformation—he meant in general conformation. He thought that there were several degrees of conformation that should certainly be mentioned, such as low heels or a tendency to speedy cutting. He himself had mentioned them in many scores of cases when called up to give an opinion on the subject. There were many instances, too, in which the veterinary surgeon was, so to speak, the owner's protector. The client looked to him for information that would be more or less as to the permanent value of the animal being examined. In reference to identification he quite agreed that the marks of identification should always be given in every certificate, because in several instances it had been brought to his knowledge that there had been a different opinion as to the personality of a certain horse, and he had to refer to his private papers so as to be able to identify the animal. As to the term "perfectly sound," he thought the word "perfectly" was always unnecessary. The term "sound" or "unsound" was quite sufficient. With reference to the term "practically sound" or "serviceably sound," he agreed entirely with Professor McCall. He found that in many cases it was quite necessary to insert those words for the information of their clients. He had before now been asked to give his opinion in Ireland upon the value of a horse as a hunter. Let them take, for instance, as an illustration the case of a horse six, seven, eight, or nine years old, certainly over five, with a spavin. Let them say it was placed far back, and had been no interference with the action of the joints. The horse flexed his hock well, and was perfectly sound. In a case of that sort they might safely term that animal practically sound. A practical veterinary surgeon could tell pretty well whether he was sound or otherwise. Therefore he thought the term "serviceably" or "practically sound" might be very advantageously used in framing certificates.

Professor Axe said he very much regretted that he had not had the opportunity of reading the very excellent paper, as he presumed it was, which had been placed before them that day. Therefore the few remarks he had to make would be addressed to the criticisms which had been addressed to the meeting by the gentleman who had spoken. Firstly, Mr. Banham had done him the pleasure of noticing some remarks he (Professor Axe) had made in reference

to the form of certificate, in which he suggested that the condition of an animal under examination should be negatively referred to. Exception had been taken to that form of certificate by Professor McCall, who had indicated that in his view of the question such a certificate as that would not be accepted. Probably that view might have arisen from the fact that Professor McCall had not adopted it. He (Professor Axe) had been in the habit of adopting that form now for something close upon twenty-five years, and he could say from an experience extending over that period that not one of his clients ever raised any objection to it, nor had it at any time given rise to any special inquiry appertaining to the animal itself. Therefore he claimed it to be a workable certificate based upon experience. Then with regard to the certificate, Professor McCall had indicated that it was desirable to express an opinion as to the soundness or unsoundness of a horse. The question of his (Professor Axe's) mind was whether they were in a position to do anything of the kind. In the first place he would ask whether they had the means of doing it: did they adopt the necessary method of examining a horse to the extent that would enable them to express an opinion as to its soundness or unsoundness? Opinions, to be ventured at all, must at all times and under all circumstances be based upon examination and upon evidence they could bring before them, or which was placed at their disposal. Then what evidence could they seek to obtain with reference to the soundness of a horse? It was simply of a superficial character: their powers of inquiry and of inspection were of a very partial character, extending simply to an examination of the exterior of the horse's body and his action, and in some cases the physiological conditions of certain organs. He felt that that was insufficient to enable any man to give a general expression of opinion as to the soundness of an animal, and it was on that account that he preferred to speak of an animal in a negative sense. Professor McCall, in speaking of the value of an animal—and he (Professor Axe) was going to draw a parallel between Professor McCall's views in one case and his views in another—speaking of their estimate of the value of an animal, Professor McCall had stated that a horse, when standing still, might appear to be worth 100 guineas, but upon a further examination of the animal he might not be worth a brass farthing. He regarded that as a parallel case. Looking at a horse's exterior they might say, "That exterior conformation indicates a certain value," and if they went no further they might pass an opinion upon the evidence before them, but would they or anybody else estimate the value of an animal by a mere examination of his contour and conformation? To express an opinion as to the value of an animal they required to know what his capabilities were, and they were no more in a position to express an opinion on the value of a horse from a mere examination of his external appearance than they were to express an opinion as to his soundness by a superficial examination of his exterior. He therefore felt that a negative certificate was the most logical and consistent one, and the one less

capable of mischief. As he came to the meeting he turned to p. 48 of the paper, and found under the form of certificate there mentioned, Mr. Banham said, "At p. 28 Professor Axe prefers to allude negatively to soundness, 'I do not detect any unsoundness,' and he considers the form preferable, as there are some diseases we cannot certify to, because we are unable to detect them by our present method of examination. This difficulty, however, might be overcome if a list of these diseases were scheduled as unsoundness, and the vendor compelled to take the animal back should any of these maladies appear within the time prescribed by law." That was the Continental custom. Continental veterinary surgeons were a little in advance of them in logic and consistency. They admitted the fact that it was impossible to examine a horse in such a manner as to enable an opinion to be given as to all its parts, and they said, "We except certain organs or diseases affecting certain organs," and in the event of those diseases existing the purchaser had a claim upon the vendor notwithstanding the certificate given. He thought, therefore, that Continental veterinary surgeons had recognised the difficulty. They said, "We examine a horse in certain parts, and so far as we are capable of forming an opinion on such an examination the horse is sound, but we do not give you a general opinion on the general soundness of the horse: we except certain diseases, the presence of which we are incapable of recognising, because we have no means of examining a horse except the ordinary means." For example, a horse might be suffering from a very serious disease, and the veterinary surgeon might certify to his being a sound horse. Next day he might have an attack of megrims without any apparent cause. The client said, "You have certified to this horse's soundness," whereas if a negative certificate had been given no such statement could have been made against the veterinary surgeon, who would be in a position to say, "Well, I have used every means laid before me, and I did not detect the presence of any disease. I have not certified to its soundness, I have simply told you that I did not detect any unsoundness in it." He still thought that the negative system of describing a horse was the preferable one to that put forward as the positive system. With regard to the word soundness, Dr. Fleming had told them, as they had been told thousands of times, that soundness meant what it expressed, soundness; but still no one had ever defined it, or rather it had been defined many times, but no two men had agreed as to a definition of it. They had it on the authority of one gentleman that it meant what it expressed: they had another expression of opinion, and that from the Bench, that soundness meant perfect. Another authority would tell them that soundness meant the adaptability of a horse to the particular purpose for which he was required. Another would say soundness meant absence of disease, and with the same breath he would say that disease might exist: that a horse may be the subject of disease, but that so long as he performed his duties well he might be considered to be a sound animal. So long,

therefore, as that want of unanimity in the definition of the word soundness remained, so long would they meet with trouble if they considered the term at all. He very much regretted that it could not be altogether abandoned in the construction of certificates. Another term he would like to refer to was "practical soundness," and with regard to that term they were in much the same position. No one had that morning attempted to define it, and when speaking of it none of the speakers seemed to have any settled idea about its meaning. They had a horse regarded as practically sound which was free from lameness, and which appeared to be capable of performing the ordinary duties of an ordinary horse. On the other hand, there were eminent authorities in the profession who, notwithstanding the existence of lameness in an animal, if it were slight, regarded that animal as practically sound; but in his (Professor Axe's) opinion no horse was in a sound state if it was lame. Therefore it might be seen that they were in a very unsettled condition as to the expression; and in consequence of what seemed to him to be a very serious difference of opinion between men of distinguished position, he had resolved to abandon the use of the term "practically sound," and had never used it since, nor should he use it again until such time as the profession had agreed upon a form of definition of the term practically sound which could be generally accepted. He thought it was a very important matter that they should formulate a definition for an expression which they were using every day. With regard to the reference to conformation in the certificates, he thought that was a highly important matter; in fact, he felt that he could not maintain his position for three months if he were to allow such a thing to pass unnoticed. For example, there were scores of horses, tall and narrow with their hocks, and with long pasterns, whose formation was seen at once to be such that if they were used for the purpose of draught, and especially if they were out of condition, nothing could save them from damaging their fetlocks, and rendering them practically useless. He considered that a man would be guilty of an act of great dereliction of duty if he were to allow a horse to pass without pointing out any possible disabilities of that kind. Another example they saw every now and then was that of animals with narrow middle, long legs and long necks, and heads ill set on, and, generally speaking, wanting in condition. Who could hesitate to say that such an animal was an ill-conditioned brute? and it would be unprofessional and a dereliction of duty to allow a man to buy a horse of that kind. Conformation was a matter that should at no time escape the notice of the veterinary surgeon. It was his duty to direct the attention of his client to such causes as were in his view calculated to give rise to lameness, or in any way to incommode the animal, or render him incapable of performing the ordinary duties of an ordinary horse. Those are all the points that I thought it right to direct your attention to. I regret that the want of time and opportunity of reading the paper has not allowed me to refer to many points in the body of it.

Mr. Olver said that when the commission was appointed by the first veterinary congress he promised to draw up certain questions for the consideration of the committee, and until illness laid him aside he had thought very carefully over the matter, and the more he thought over it the more he became convinced that they would never entirely agree upon the question of soundness. If they argued till the time of the next Association meeting they would still be as wide apart as they were that day. He thought, however, that it was for the benefit of the profession that those matters should be discussed, and as his name figured frequently in the paper he desired to make a few remarks. He would first direct attention to the question as to veterinary certificates. For several years past he personally had very rarely indeed ever given a certificate. He had come to the conclusion, and he had found practically that it was very much better, not to give formal certificates at all. In almost every case he wrote a letter to his client explaining to him the condition in which he found the animal, and since he had done that he had scarcely ever had a subsequent inquiry. He thought that in an ordinary way that was very much the best thing to do, viz. for a veterinary surgeon to say that he had examined the animal and found certain defects which were technical unsoundness, but which would not interfere with the usefulness of the horse. In that way the objectionable words "practically sound" might be done away with. The veterinary surgeon by adopting those means thoroughly explained himself and his client was satisfied, and greater satisfaction was given to the vendor as well as the vendee. That at least was his experience, and he was inclined to think that if they were to leave out some of the formalities in the ordinary certificates and used letters, there would be less trouble in the matter. Then as to the form, he quite agreed with what had been said by Professor McCall and Professor Axe, that it was impossible for them to do justice to their clients if they took the horse as he stood. He should like to know what the position of a client was when a veterinary surgeon passed a horse as sound when it was perfectly patent that he could not stand work? He (Mr. Olver) thought that it was an injustice to their employer if he was given a certificate of soundness without being told that his horse would not stand work afterwards.

Professor Walley said that the paper went over so much ground that everyone would agree that it was utterly impossible to look over even one quarter of it. So far as he was personally concerned, the older he grew the longer he felt the necessity of being cautious what he said in any certificate he might give. He quite agreed with Mr. Olver, his practice was one that he (Professor Walley) frequently adopted, and always when he could he wrote a letter and explained very briefly and simply what he found in connection with the animal. A certificate from a veterinary surgeon in some parts of Scotland was often looked upon really as a warranty of a horse that he must be sound; he was warranted, and over and over again he had heard the expression, "The horse is warranted, and

is passed by So-and-so." Those gentlemen had a purpose, and it was done mostly by men of an ignorant class of horse dealers, but they knew it was not worth the time taken up in stating it. It was well, as far as possible, to avoid giving certificates. Of course there were occasions when a man sent a horse under dispute, and a certificate must be given ; but it was far better to take him as a friend as well as a client, and point out to him quietly what was found to be wrong with the horse, and what would probably in the future damage the horse from those particular defects. There were a great number of slight defects of which they might say to him, "There is so-and-so here, and so-and-so there, but probably it will not hurt him." But if they neglected to point them out some other fellow, probably a groom knowing where they were, would point them out. He maintained that they had no right to pass over any defect at all. Certainly there were cases where one felt inclined to pick out the most glaring defect and stick to that. He thought that they should point out everything verbally if they did not put it on their certificates. In respect to giving an opinion as to conformation, he certainly agreed with what had been said, that if a veterinary surgeon examined a horse for a client, and particularly if that client was a man comparatively ignorant of horses, as they were in some cases, if he examined a horse with bad conformation, with toes turned out, and so on, he should certainly mention it to the client. If he did not, and anything happened to that horse, naturally the client would say, "Why did you allow me to buy that horse?" Only the other day a man came to him to examine a horse. He said that the horse was not worth a rap to him three months after he had bought it, and he gave £80 for it. He thought that they should always guard their clients against all the worst defects of conformation. What was done as to this matter on the Continent, as far as he could understand, seemed far superior to what we do here, which was that no warranty held good except in reference to any hidden defects. He thought that was done in Paris.

Mr. McGill said that in Paris they did not grant warranties at all ; the veterinary surgeons were only called in to find defects.

Professor Walley said that he considered those hidden defects were of very great importance, and he thought that a veterinary surgeon when examining a horse for soundness frequently overlooked points that he had no right to do. He might be seen examining a horse and simply looking at his lips, and never looking at his mouth at all, except to see his age. He never opened his mouth to smell the odour or anything of the kind, and perhaps he would never look at the *mucous membrane* to see if there was anything unnatural about it. He never picked up his feet, or anything of that kind. If they did not do these things they were certainly to blame, and it might be that they were liable for the neglect. As to the form of certificate that he had been in the habit of using, his form was, "I have this day examined such and such a horse ; he is such and such an age, and in my opinion is sound or unsound, as

the case may be." If he found a glaring defect he said it was unsound. If it was only a slight defect he said it was sound with the exception of such and such a thing. Then he pointed out at the bottom what was the defect that he found, mentioning it as not of great importance, and as not likely to interfere with the usefulness of the animal. He maintained that every veterinary surgeon in the kingdom as he went on in life would adopt certain methods, and they would be largely formed by the characters of the clients he had to deal with, and largely upon the character of the horses he had to examine, whether they were high-class, middle-class, or whatever they might be. He could then form a definite line of his own. He thought they should be extremely careful not to leave a loophole for their clients to land them in any difficulties, and to be as guarded as possible in all their examinations.

Mr. Greaves said that he had listened to the observations of the various speakers with very great pleasure. He had read the paper produced by Mr. Banham, and he was sure Mr. Banham deserved all their best thanks for the labour he had taken in compiling that valuable paper, in which he had placed in a new shape the opinions of many experts, most of which would be of great service to them in after life. As they advanced in life their opinions became more and more matured, and they became more and more careful in what manner they wrote their certificates. As to difference of opinion on that subject, it was not to be wondered at. No doubt each man in the room had seen cases where men were balanced in their own mind, and who said, "Shall I reject this and call him unsound, or shall I pass him and call him sound?" A horse might have one hock a little larger than the other, but his regular action in each hock was perfect. He might be seven years old. Was that enlargement or disease, or was it the natural state of things? He had seen cases where he dared not say exactly which it was. In every case of that kind he thought it was best to try and throw some responsibility on their employers, and say, "There is one hock larger than the other; you can please yourself whether you buy it. I can tell you there is a little risk about it; but in my opinion he will not go lame with the work he has to do, but a little twist in a moment may produce lameness. Now, I leave it to you to buy him or leave him." When they could get the halter round their employer's neck as well as their own the responsibility was divided; and he thought that when they could manage to do that in reference to conformation it was a good thing. He had been in his profession fifty-five years, and had seen many cases that had deceived him. He remembered the case of a horse that a client was buying. He thought that of all horses he ever saw he was the most unlikely to stand work, as narrow-chested, ill-going a brute as ever was seen, and one he was certain that either Professor McCall or Professor Walley would not have recommended, but on the contrary would have said, "Do not you have it." He happened to lay stress upon it, and he said, "This horse cannot endure work;" and strange to say the gentleman bought him, and afterwards said, "I

can work him seven days a week for six months together, and he never tires." In another case a most likely horse to stand work, with good formed legs and carcass, and in every way a likely horse that a good sensible man would buy, went bad in a short time, and he was not worth a shilling. The cleverest might be taken in at times, but in every case where there was a doubt he thought they should endeavour to throw some of the responsibility upon their employers.

Mr. Hunting thought the question they were trying to elucidate was how best to examine a horse without quarrelling with their clients, to do good to themselves and keep outside the law. There were, however, many difficulties in their way, and the first difficulty that occurred to him was one simply of diagnosis. They heard men say very often that it was a perfectly disgraceful thing for five men to put one thing down, and six men another; for one to speak of it as one thing, and one as another. That did not at all follow. Dr. Fleming had that morning said that any man ought to know whether there was a spavin present or not, that there could not be anything different about it. As a matter of fact there were a lot of things different about it. Spavin, so far as external diagnosis went, was an enlargement of the hock, varying in size. It was not a different colour from the rest of the animal. The only possible thing they had to go by was some slight difference of curvature, of a slight difference in line. He thought that any man had a right to dispute some cases of it, and any two men could not be blamed for disputing about its mere existence. And then having discovered some of those facts, they had to form a prognosis about it. They had to bear in mind that they did not want to deprive their clients of buying a good horse. What was the prognosis? Would he go lame? No man living knew. A man having a large practice amongst horses might arrive at a conclusion with tolerable correctness, but as a matter of fact no man living knew whether a spavin was going to lame the horse or not. Having made the diagnosis and prognosis, the next difficulty was the expression of opinion: they had to satisfy their client and the law, which was a very difficult thing to do. The somewhat academic discourse which Professor Axe had delivered seemed somehow to overlook the fact that there was a client and a law. That they must surely remember. The law said so-and-so, and it was necessary to trim in between the law and the client. To get over that difficulty there were a few things to be noticed, and especially the terms and expression used in the wording of the certificate, "sound" and "unsound." They could not alter the English dictionary if they would. They had got to use the term "sound" or "unsound," and it was no use dividing them into four or five or any other number of divisions; the smaller number of divisions they made the better. There was always some miserable disease which would crop up on the marginal line between the two divisions; but between them there was really a very large margin, and the horse might be put into one or the other. He therefore held that they

could cover up that margin a good deal by the use of the term "practically." He did not know any word more legitimately useful and in accordance with common sense. Mr. Walters had suggested the word "serviceably." He thought that that would be a better word, but, however, it was a matter of taste. As to the form of certificate, negative or positive, he really did not know what Professor Axe's idea might have been in using that term except what it suggested, that he was trying to evade the law. But the law could not be evaded, or it was a very difficult thing to do so, and whether he wrote a negative or a positive certificate it could not much matter, for there were such things in law as interrogatories which had to be answered. It did not matter whether they answered them negatively or positively. The law required them to mention all the defects. If they did not they did not give their client the opportunity of making up his mind. If they told their client that there were certain things that might or might not do so-and-so, it was his look-out if after that he bought the horse. Professor Axe had said that they could not mention them all, but if they had shown ordinary and due care they were all right. The mere fact of a horse having some disease which they could detect with ordinary care relieved them from any responsibility. As to conformation, he certainly held that a veterinary surgeon had nothing to do with conformation, unless he was prepared to say that it was going to be the immediate cause of lameness. Professor Axe had told them that it was a disgraceful thing to pass a horse with a long pastern, but in practice they knew that some people liked that. Then as to marks of identity, he believed they should only put in sufficient for purposes of identity. A man had no right at all to stick conformations or peculiarities in among these notes of identification ; by doing so he simply came in and interfered between buyers and sellers. Professor McCall had urged the necessity of keeping a copy of the certificates, either in the form of a letter-book or a counterfoil. He (Mr. Hunting) did not keep a letter-book or a counterfoil, and that is why he should use certificates. To sum up the whole matter, what the veterinary surgeon had to do was the best in every case, and to do that he wanted to know his business. A man wanted to know something of law so as to keep outside of it, and something of men so as to understand both the buyer and the seller, and lastly he had to please himself, and he thought the best way of doing so was to please himself in the way Shakespeare recommended :

"To thine own self be true,
And it must follow as the night the day
Thou canst not then be false to any man."

Mr. Parr said he regretted that there was not a greater number of younger veterinary surgeons present to hear so interesting a discussion, as he thought they would have profited a good deal by it. In reference to the terms "sound," "practically sound," and "unsound," several of the gentlemen who had spoken considered "practically sound" a good term. He considered it was the worst

term possible for a veterinary surgeon to put on his certificate. It opened a vast field, for what one man would call "practically sound" another might call "decidedly unsound." He had very strong opinions upon the term "practically sound," as he thought it ought not to be used at all. If a horse was sound they ought to call him sound; and if he had certain defects which they considered of any practical importance they ought to mention those defects; but to simply use a vague term and say "practically sound" he considered was not the right thing to do. At present there were a great many horses sold at auction sales, and the veterinary surgeon was often called in to examine those horses so far as the description went in the catalogue. A very common description of hunters was that of "a good hunter." He had seen a great many differences of opinion as to what a good hunter consisted of. Some people said it only consisted in wind and eyes, and if they were good there they must pass them as good hunters. Then there were those who were curb-biters or windsuckers, and who were still described as good hunters. As to curb-biting, he was rather inclined to say that that answered the description of a good hunter; but for windsucking he did not think he could pass a horse as a good hunter. All those were points that had not been touched upon by any of the gentlemen that had spoken, and he should like to have some information about it. As to conformation, he thought that a veterinary surgeon ought to take into consideration the conformation of the animal he was examining; he thought that was part of his duty. He was often asked not only about conformation, but as to whether he had good shoulders, and what sort of thighs he had, and a lot of questions like that which he was bound to answer. It was a very nice thing, no doubt, as Mr. Greaves had suggested, to throw the onus on their employer, saying, "This horse has got so-and-so, and you must take the risk;" but it must be remembered that they were employed to give an opinion, and their clients wanted that opinion, and they did not want them to say, "It may be this way or that, and you must take the risk of it."

Mr. Byrne said that he held a strong opinion upon the subject of conformation. There were many cases in which the veterinary surgeon had to act not only in his professional capacity, but as an expert on horses. But veterinary surgeons as scientists had nothing whatever to do with conformation if they performed their duties in a proper manner. The question of conformation in the horse was a very large one indeed, and he did not think the veterinary surgeon was warranted in pronouncing an opinion upon it. The veterinary surgeon had simply to determine whether a disease existed at the time of his examination or not. He would like to know something about the question of grunting, for he found some were rejected for grunting, which after a certain time were found to be perfectly sound. He thought that a horse that grunted under these circumstances could not be passed as sound, but he should like to hear the opinion of some one with a rather larger experience on the subject.

Professor MacQueen said that from his observations and practice he had learned that more care was exercised north of the Tweed than south, and that to a large extent was the cause of the dispute in England. He thought that a great many men in examining a horse approached the subject with too much trepidation. They seemed to be in fear and trembling as to whether they should use the word "sound" or "unsound." He held that when a veterinary surgeon was employed to examine a horse he was asked to give an opinion for a fee, and if he carried his duty out as he should he would not have any difficulty. He thought the veterinary surgeon nowadays was too conscientious when he examined so carefully that he could not express an opinion without covering his own retreat. If they took an engineer or any other scientific individual, he would give an opinion on the payment of a fee. Of course it would be said that that was a different subject, but how could it be that some gentlemen could incorporate questions of conformation in their certificates when they could not say that that horse was sound or not? How could they say that a horse of so-called bad conformation might be sound? Why not extend that confidence a little farther, and say that the horse was unsound, instead of saying it was of bad conformation, and they failed to detect any unsoundness? As to the wording of certificates, he thought that the word "practically sound," as used in a certificate, was only a base subterfuge, because a horse was either sound or unsound. It is convenient to describe a horse as "practically sound" to meet the wishes of buyer or seller, but it was certainly not good business on the part of the veterinary surgeon who examined that horse for a fee to certify that it was "practically sound," when he did not know whether it would remain so or not. It seemed to him that it would be better to abolish the phrase "practically sound" altogether, and at the college they no longer described a horse as "practically sound." As to the use of the word "unsound," he thought that a veterinary surgeon ought to be capable of saying that a horse was unsound. He did not think it was necessary that he should simply say, "I fail to detect any unsoundness;" he thought that something more was expected of a man. After training and after experience he should be able to go a little farther than this; he should be able to answer the question in the affirmative. They were only human beings, and were not asked for anything extraordinary. "Was he sound or unsound?" He was sound. In regard to the use of the term "unsound," he believed with Professor Walley that a great many cases could be got over by not writing a certificate at all. Very frequently he never used the words at all, but simply described the condition in which he found the horse; and if anybody was not satisfied and required further information, he was quite prepared to give it. If the horse happened to have certain defects he simply described them as they appeared to him, and said that it might or might not be fit for work as the case might be; but he thought that the phrase "practically sound" was misleading, therefore he simply stated what he thought wrong

with the animal, and that very often met the case. He really thought that a veterinary surgeon ought to state his opinion in the affirmative in examining a horse. There was very little occasion for a difference of opinion, notwithstanding what Mr. Hunting had said. If they began on the various parts of the horse and went over them, there were only a few parts where there was any liability for dispute, and one was the particular part that Mr. Hunting had mentioned, the hock, where no doubt there was great room for dispute. He also thought that no veterinary surgeon should interfere with conformation at all, it was quite without his province, except when specially asked for it. He believed that a large amount of the knowledge regarding conformation was a mere matter of sentiment. They must remember that horses were for different purposes, and what might be good conformation in one might be bad in another. It was a remarkable fact, which was attested to by Mr. Greaves, that some of the worst formed horses were the best workers and lasted the longest.

Mr. said that as for terms on their certificates, it was only an expression of their opinion, and they were asked for nothing more; and whether they gave it in the affirmative or negative, it did not matter much, as long as they used the proper care. As to the term practically sound, he thought it of great importance that they should come to some decision in reference to what it meant. If they took to rejecting horses for slight blemishes, and call them unsound, he thought that they would be able to find very few horses that they could call sound. With most horses these slight blemishes were of very little importance, and not at all likely to cause the animal to come to grief. In such cases they were of equal value to their owners as if these blemishes did not exist at all. In matters like that it was his custom to send explanatory letters, mentioniug the slight blemishes he found and giving full explanation of them. He should like to get some information concerning intermittent roaring, as he had seen several horses rejected for it. He had one in his possession for a year and a half. She made a slight whistling noise, but she could be galloped till she dropped down dead, and she would make no noise. In cases of that kind he thought it was very difficult to accept the responsibility of giving an opinion as to soundness or unsoundness.

Professor Pritchard said he had nothing to say upon the subject which had not been said over and over again, and written over and over again. He had sat there all the morning and afternoon, and, without saying anything at all to interfere with the feelings of anybody, he might state that he had heard nothing fresh. The opinions expressed on either side in connection with any one point that had been discussed were correct, whether they were for or against.

Mr. Bell said that with regard to the words "practically sound," he knew of three cases to which his attention had been drawn during the last six weeks, where animals which had been

passed as practically sound were at the present time lame. He thought it was a very dangerous thing to pass any horse practically sound, as there were many defects which might deceive them. He would therefore caution younger men from using the term "practically sound:" it was much better to say it was sound or unsound.

Professor Pritchard said that Professor Axe had stated that he would not pass a horse "practically sound" that was lame. Their friend Mr. Bell had just said something to the same effect. Now when Apology started for the Leger she did not exactly go on three legs, but she won easily. If the word practically was to be used was she to be called "practically sound"?

Mr. Banham, in reply, said that there was very little for him to say in response to the remarks made upon the paper. He certainly had expected to hear a little more as to the fixing of terms to be used with their definitions in the certificates. He did not see very much difficulty in settling it, and he thought it would be better if they were fixed, and considered it was the duty of the profession to do so. If they only employed the terms "sound" and "unsound," it appeared to him that most horses would be rejected, as there were very few sound horses in the kingdom. Mr. Bell had just told them of cases where horses had been certified as "practically sound" and had afterwards gone lame. Suppose the veterinary surgeon had certified these animals as "sound," and they had gone lame within a month, or say within twelve hours, it would be quite possible that the professional man might have given a correct opinion when they were examined by him, and it would not have altered the case in the least whether the horse had been called "sound" or "practically sound." He thought it was a matter of education for the public, who expected too much of the veterinary surgeon. There was no other profession, so far as he knew, that had the responsibility cast upon it that the veterinary surgeon had, and he should like to see some method devised whereby they could relieve themselves of some of that responsibility; and unless they came to some arrangement among themselves as to the terms and definitions to be used by professional men, and the classes into which certain diseases should be placed when diagnosed, he thought they would remain in the same state as they now were. He did not see any reason why they should not have a scheduled list of all the diseases which existed—which were not many; and if a horse was affected by any of those diseases, that he should be placed under a certain class which might be fixed by the profession. There was no reason why the terms sound and unsound should not be used alone if the profession so decided it; but as a matter of practice he thought the term "practically sound" a very useful one, when it was used properly—that was to say, when a definition was given of it which was recognised. He would have liked to have a committee formed, consisting of say a dozen or two dozen of the most influential and experienced men of the profession, to earnestly consider

the subject and place a report before the profession to vote upon the various points, each to be settled by the majority of votes for or against each point. It was not a very large matter to schedule the various diseases. Of course they would never be able to make all see the matter in the same light: they would always have differences of opinion as to the presence or absence of disease; but what he would like to have fixed was, given a certain disease existed, that one veterinary surgeon should not be able to give a "sound" certificate and another an "unsound" certificate. He had unfortunately frequently known that to be the case, and had seen it over and over again, veterinary surgeons both agreeing that a disease was present, and one calling it sound and the other unsound. His own opinion was that no horse with any disease about it could be called sound, but if he had a disease which according to the best judgment of the examiner would never interfere with his usefulness, he thought it a very good case to use the term "practically sound." So far as the form of the certificate used by the profession was concerned, he considered it of little importance so long as it was a faithful and truthful opinion, although from a business point of view perhaps it would be as well to have a recognised form. As to conformation, his opinion was that it ought to be pointed out, particularly when it was likely to be detrimental to the usefulness of the animal. He knew from his own practice that he was often asked to go long distances perhaps to see horses that his clients had been written to about. They had never seen them, and simply wrote to say that they wanted the horses examined, and if they were "sound" they would buy them, and if not they would leave them. Suppose, now, that with an order from a client of this kind we found a horse had some little defect, but otherwise it was a useful animal, would it be quite fair to the client to pass the horse "sound" without mentioning the defect? We should say it would not. On the other hand, suppose it to be "sound," and yet it had such a twisted joint that in the opinion of the veterinary surgeon he could never get over a fence, or even hack without hitting himself, he (Mr. Banham) did not think that the client would be satisfied even if he received a "sound" certificate, when he received such a twisted-legged horse home as a hunter. If he sent a veterinary surgeon to examine a horse for him that he had never seen, and told him to tell the dealer to send it if "sound," and the veterinary surgeon gave a "sound" certificate and sent him home with such twisted pasterns that few people could ride on him, he (Mr. Banham) thought it would be unfair to the client not to point that out before the purchase was made. He thought those were the only points on which he need make any remarks. He should certainly, however, have liked to have some rules drawn up. If some of the heads of the profession were to try, he was sure they would find it a very easy thing to do. Mr. Banham was rather disappointed at the results of his paper, although he thanked the members for their appreciation of his efforts.

Mr. Hunting thought it would give some practical effect to their meeting if they tried a resolution on the use of the words "sound and unsound."

Professor Walley said that that would only be the expression of the opinion of the few there present.

The *Chairman*, in closing the discussion, said that there was a wide difference between the examination of a horse prior to purchase and the examination of one after purchase. Prior to purchase, as a rule, they were accompanied by their client. For his own part, he had never yet bought a horse for anybody unless the owner was present. He gave his opinion both as to conformation and as to soundness, both practical and otherwise; and he thought that in the arrangements prior to purchase, when both the seller and buyer were present, the term practically sound was a very useful one. He might say, "This horse is so-and-so, he has a thick leg, and this and that, but I think he will suit your purpose." He might say he was asked so much money, and he (Professor Williams) might then be able to buy him for a little less money. It was a matter of business between the buyer and seller, the buyer being advised by the veterinary surgeon. The term "practically sound" came in very usefully there, but after purchase if a veterinary surgeon was called in then, the certificate must be a positive one; no negative certificate would do then. He believed that a negative certificate would very quickly be made into a positive one, or made of no use whatever by the cross-examining barrister, and it would tell against any man who had written one. After purchase the veterinary surgeon was an expert, and it was necessary to say whether the horse was sound or not; but before purchase was a different matter. With regard to conformation, they must exercise their common sense. If the conformation of the animal, in their opinion, was calculated to interfere with the usefulness of the animal, that certainly amounted to unsoundness. It might not be so at the moment, but it would be so when the animal was put to work; and he should not hesitate for a single moment to say that it was an animal of such a conformation. As to law, the law was very well expressed upon that point. A friend of his made an awful mistake, and he consulted a solicitor, who said that law was made for the wise man, and not for the foolish. If a man passed a horse with a conformation such as to unfit the animal for any useful purpose, he certainly would not be called in again. He did it with his eyes open, and should take the consequences. He thought Mr Banham had taken a great deal of trouble in placing a lot of useful information before them—information much more useful than if it had emanated from Mr. Banham himself. He (Professor Williams) felt highly indebted and very thankful to Mr. Banham for the great labour he had expended in preparing the paper.

Mr. Hunting said he wished to propose—"That this meeting is of opinion that the term sound or unsound should only be used in the writing of certificates, and that indefinite conditions and harmless defects be referred to specially."

Mr. Bell said he had great pleasure in seconding that proposal. He did not, however, think that the words "indefinite conditions" were needed.

Mr. Hunting said he would leave out those words, and simply say "harmless defects."

Mr. Hughes asked Mr. Hunting to say whether by "harmless defects" he meant sound or unsound.

Mr. Hunting said he only wanted to have the words sound and unsound used.

Mr. Banham thought that before they voted on this matter they should know what Mr. Hunting's definition of the term "sound" was. Another gentleman asked for a definition of "unsound." If they said they failed to detect any unsoundness he should like to know what it was that they failed to detect.

Professor Pritchard thought that, as far as that matter was concerned, there was no difference between the two certificates. He proposed an amendment—"That it does not go forth from this meeting that we are bound to the word "sound" or "unsound."

Mr. Hunting said that that was not an amendment.

Professor Pritchard then moved—"That it be not put to the meeting."

Mr. Hughes seconded the amendment, which was put to the meeting and carried, 14 voting for it and 6 against.

Professor Walley said that there was one duty which was always performed in the Association, and that was to pass a vote of thanks to the writers of papers. Those who had had to prepare papers knew the trouble, expense, and loss of valuable time it incurred; and he therefore had very great pleasure in moving—"That a vote of thanks be given to the essayist on that occasion for the paper he had compiled for them."

Mr. Mulvey seconded the motion, which was carried.

Mr. Bell moved a vote of thanks to Professor Williams for the manner in which he had conducted the business of the meeting.

Professor Pritchard seconded the resolution, which was unanimously agreed to; and the chairman having acknowledged the compliment the meeting terminated.

ROYAL AGRICULTURAL SOCIETY OF ENGLAND.

Monthly Council, Wednesday, February 4th, 1891.

The Earl of Ravensworth, President, in the chair.

Among those present were—

Trustees.—H.R.H. the Prince of Wales, K.G., the Duke of Richmond and Gordon, K.G., Earl Cathcart, Sir Nigel Kingscote, K.C.B., Sir A. K. MacDonald, Bart., Earl of Powis, and Mr. Dent.

Vice-Presidents.—H.R.H. Prince Christian, K.G., Earl of Lathom, Lord Moreton, Sir John Thorold, Bart., Mr. Walter Gilbey, Mr. C. Whitehead.

Veterinary Committee.

Sir John Thorold reported his election as chairman of the year. Professor Brown had presented his usual report, and the annual report of the Royal Veterinary College, which the committee recommended for publication in the Journal. The subject of pink-eye in horses had been discussed in reference to a note on this disease by Professor Axe, written for the next number of the Journal. The examiners in Practical Cattle Pathology for the diploma of the Royal College of Veterinary Surgeons in 1890 had reported that the following gentlemen, placed in order of merit, had attained the greatest distinction:—(1) J. E. Row, 2, Torbay Park, Paignton, Devon; (2) F. E. Place, Church Street, Southwell, Notts. The committee recommended that the Society's large medal be given in silver to Mr. Row, and in bronze to Mr. Place. The committee recommended the appointment of Mr. Lewis P. Rees, of 5, Lammas Street, Carmarthen, as Provincial Veterinary Surgeon for Carmarthenshire.

Professor Brown's Report.

Pleuro-pneumonia.—During the four weeks ended January 24th outbreaks of this disease have been dealt with by the Board of Agriculture in the counties of Durham, Essex, Lancaster, Surrey, York (W. R.), Aberdeen, Ayr, Fife, Forfar, Linlithgow, and Midlothian. By order of the Board fifty cattle affected with pleuro-pneumonia in various stages were slaughtered, as well as 510 other cattle which had been in contact with the diseased animals or otherwise exposed to infection.

Anthrax.—Of this disease fifteen fresh outbreaks were reported in the four weeks in the counties of Bucks, Cumberland, Hants, Hunts, Lincoln (Kesteven), Norfolk, Rutland, Somerset, York (W. R.), Aberdeen, and Dumfries. In these outbreaks thirty animals were attacked, eight diseased animals were killed, fifteen died, and five recovered.

Swine Fever.—This disease is again decreasing, and has been so for some weeks. In the four weeks of November the outbreaks reported in Great Britain amounted to 411, or just on 103 per week. In December they fell to 313, or about eighty-six per week, whereas in January they still further declined to 248, or sixty-two per week. In these January outbreaks 1968 pigs were attacked, 864 diseased pigs were killed, 900 died, 265 recovered, and 412 remained alive when the last published return was made up.

Rabies.—There have been six cases of this disease, all of them in dogs, reported in the month; one of these occurred in Hants, two in Kent, and three in York (W. R.).

LANCASHIRE VETERINARY MEDICAL ASSOCIATION.

A QUARTERLY meeting of the Association was held at the Blackfriars Hotel, Manchester, on Wednesday, 10th Dec., 1890.

Present.— Mr. W. A. Taylor, President; Messrs. Wm. Hunting, Peter Taylor, T. Greaves, J. D. Wolstenholme, E. Faulkner, T. Hopkin, P. Moore, A.V.D., W. F. Shore, Alex. Lawson, S. Locke, W. Dacre, E. C. Edwards, — Carter, E. S. Gubbin, T. Hart, T. Briggs, E. Noar, — Kilsell, H. Ferguson, — Bridge, J. Umson, A. Leather, W. Hall, A. Michaelis.

The members sat down to tea at five o'clock, after which the minutes of the last meeting were read and confirmed.

Letters of apology were read from Professors Williams and McCall; Messrs. Henry Thompson, R. Hughes, G. Gartside Mayor, P. Pike, Jos. Abson, T. Leather, W. Whittle, E. Neild-Siddall, and C. W. Elam. A letter resigning his membership was read from Mr. J. Sutcliffe Hurndall. This was accepted with regret, it being well known that only distance prevented Mr. Hurndall from attending.

Mr. A. Leather nominated for membership Mr. Roberts, M.R.C.V.S., of Liverpool.

The *President* explained why no action had been taken with respect to the resolution of last meeting appointing a sub-committee to draw up a resolution on the new Pleuro-pneumonia Act; and it was proposed by Mr. P. Taylor, and seconded by Mr. Locke, that no further steps be taken in the matter.

The *President* then called on Mr. Hunting to read his paper on "The Use and Value of Position and Action as indicative of Lameness."

Mr. Hunting commenced by a review of the normal position of the horse's limbs when at rest, and of their action during locomotion. He then referred to the various alterations which are noticed in cases of lameness, and asked, What is the value of position and action as indicative of lameness? He concluded that their value was over-estimated, and that there were not many lamenesses which could be definitely diagnosed by action alone, nor even by peculiarities in position of the limb. From the general question he passed on to consider in detail the various regional lamenesses, and his conclusions may shortly be summed up as follows:

Shoulder.—The position of the limb and its action does indicate the region affected, but it does not definitely show the seat of injury. Very similar alterations of position and action result from such widely different actions as sprain of the flexor brachii, of the extensor brachii, of the spinata muscles, or even of the joint. In each of these we have indications suggesting some injury to the region of the shoulder, but the whole of the muscles of the region are so interdependent on each other that failure of one affects all the rest, with the result that the shoulder cannot be raised, and the proper angle of its bony elements be maintained.

Elbow.—The excessive dropping of the leg during progression which is noticed in these cases is not diagnostic. It is seen in injuries which often have their seat higher up.

Knee.—The essayist expressed his distinct disbelief in any definite form of action being diagnostic of lameness affecting this joint, unless it was of such a degree as to visibly prevent flexion. Digital lameness possessed no special action, and caused no peculiar position of the limb which was not seen in cases affecting other parts. When ringbone was well developed there was a clear want of motion in the joint which was indicative. He did not think that the difference stated to exist between the movements of a leg affected with ringbone in front and behind were always noticeable. In the fore-limb ringbone caused the foot to come to the ground heel first, but he had not noticed that in the case of a hind foot so affected the toe first came to the ground.

Hip.—Lameness of this region, it was pointed out, was common enough in cases of fracture which, when accompanied by no displacement, ought to show a definite action, if such ever existed. There is certainly something almost indicative in the action of hip lameness, but the same condition—the limb being carried all in a piece—is seen in some acute foot lameness. Mr. Hunting characterised such action as well described by a horsey man who was looking at a case of lameness when he said, "Well, he's lame at one end or the other." We can only get about as far as this by action alone; we can suspect either foot or hip, and then must resort to close local inspection to determine the exact seat.

Stifle.—Here is a joint, said the essayist, which can by no possibility give rise to diagnostic action by any diseased condition. Any peculiarity of movement of the hind leg which alters the motion of the stifle must also alter the action of the hock. Both hock and stifle must move together, as the bones which form them are so connected by tendons and ligaments as to retain always a similar relation. Flexion of hock must be accompanied by flexion of stifle, and the amount of flexion is always equal in both joints. When any of the connecting structures are divided or paralysed, then we have very remarkable movements of the limb which are truly diagnostic of the lesion—as, for instance, in rupture of the gastrocnemius muscle or of the flexor metatarsi.

Hock.—Many men profess to diagnose hock lameness by the action, and they are very often right, but there is no definite action peculiar to a lame hock. As far as motion goes it might be the stifle, but as in ninety cases out of a hundred the stifle is not affected, it is a fair practical guess to say it is the hock. But even putting aside the stifle as a source of error in diagnosing hock action, Mr. Hunting seemed inclined to doubt whether movements of the hind leg could be depended on as indicating the region affected, and he related two cases of injury to the pedal bones which had been credited to the hock until a *post-mortem* examination revealed the true seat of lameness.

The Foot.—This region, it was allowed with qualifications, did

give some indication by its position or movement of lameness affecting its structures. The position of "pointing" was indicative of foot lameness, and there was a peculiarity more easily detected than described, which enabled some men to very positively locate the lameness to the foot, more especially in the fore-limb.

Discussion.—Mr. Dacre opened the discussion by exhibiting a morbid specimen showing the effect of a puncture on the fetlock-joint. Some pointed object had entered behind the fetlock-joint of a fore-leg and set up acute inflammation. As a rule pain in a joint resulted in flexion, but in this case the horse seemed to be in least pain when the foot was firm and square on the ground in its normal position. Motion was accompanied by great pain.

Mr. Peter Taylor discussed the various lamenesses, and gave a general support to the statement that action was not of itself very indicative of the seat of lameness. He said the longer he lived the less positive he became in his diagnosis, and he recognised that very often the most expert practitioner might see a lame horse whose action taught nothing, either as to the seat or nature of the injury. He impressed the necessity of examining the foot carefully in all cases. As to the statement that 80 per cent. of fore-leg lameness was in the foot, he was not prepared to admit it, except with the important qualification of age—it held in aged but not in young horses. He considered the hock lamenesses were the most difficult subjects in the whole list. Not only could action not be depended on as diagnostic, but even the structural changes and local signs were not always to be relied upon, as was shown by the very diverse opinions expressed by different men on the same case.

Mr. Thos. Greaves said that in elbow lameness he looked upon one peculiarity as diagnostic, viz. that the animal could not back, and when compelled to do so the limb was dragged. He had hoped to hear more on the subject of bent fore-legs in cab and 'bus horses, which he suggested were due to atrophy of muscle.

Mr. Edwards said time would not permit him to go through the long list of lamenesses, but he should like to mention a condition which, in his experience, was an instance of a specific motion indicative of lameness in a certain part. In cases of injury to the hind foot the animal moved with a long step, the foot being placed far under the body.

Mr. Faulkner expressed his general agreement with the conclusions of the paper.

Mr. Wolstenholme discussed the question of "bent leg."

Mr. Briggs agreed with Mr. Greaves that the inability of the horse to back was very indicative of elbow lameness. In reference to the cases of "dropped" shoulder, he believed many of them were due to sprain of the serratus magnus.

Mr. Moore also spoke, and related cases which had come under his notice in the Army Veterinary Department.

Mr. Hart said he thought he had noticed a peculiarity in the mode of moving the limb in ringbone—the heel was raised from the ground preliminary to moving the foot. When ringbone

affected the hind leg the foot was extended, but just before being placed on the ground was retracted a little. This movement he thought special to ringbone.

Mr. Locke, in the course of his remarks, referred to the peculiarity of action in painful lameness of the foot. The foot no sooner touched the ground than it was removed with a sharp upward lift indicative of pain.

Mr. Hopkin related some cases of injury to the shoulder caused by horses falling in the shafts. On the animals being raised the limb was found powerless, the shoulder "dropped," as in cases of muscle-sprain, but recovery was rapid—so rapid that the lesion could not have been one of serious structural injury. He thought the cause was pressure of the shaft on the elbow as the animal lay on the ground. In elbow lameness he certainly had seen the horse stand with the foot of the injured limb behind the level of the sound one. As to hind-leg lameness, he was not prepared to accept Mr. Hunting's statement that hock and stifle must always act together. He thought he had seen cases in which these joints acted so independently of each other as to allow a decidedly altered motion of one of them.

Mr. Alex. Lawson related a case of Carpitis, in which the action of the joint clearly indicated the seat of lameness. In all cases of lameness which were not evidently due to some visible injury, he agreed with Mr. Taylor that we should examine the foot. As to hock lameness, some were very easy, because we had definite local changes, as well as the action of the lame animal, to guide us; but there were other cases in which there was nothing to be seen, but which were undoubtedly due to changes in the joint. In such cases the absence of other disease or injury was, in conjunction with the action, our sole guide to diagnosis.

Mr. Hunting, in reply, said he had first to thank the members for the patient hearing they had given him, and next for the very interesting discussion they had raised. He had hoped to elicit from their practical experience some observations which would assist them all in diagnosis, and he was glad to have heard two or three which he should test at the earliest moment. Mr. Edwards had mentioned the long step taken by a horse with an injured hind foot; Mr. Briggs and Mr. Greaves both referred to the difficulty of backing a horse with elbow lameness; Mr. Hart noted a peculiarity in movement of the foot in ringbone, and Mr. Locke the snatch of a foot from the ground when that organ suffered from a painful injury. All these were matters which deserved attention, and perhaps there were still other little matters of a similar kind which would be brought to mind by this discussion. Mr. Briggs had suggested sprain of the serratus magnus muscle as the cause of many of the "dropped" shoulder cases. This was new to him, and seemed so likely to be correct that he should not forget to verify it whenever he got the chance. Mr. Greaves and Mr. Wolstenholme had referred to "bent legs." Had time permitted he should have liked to enter more into this

question. His own belief was that nearly all cases were traceable to tenderness in the feet, and that it always came about by passive shortening of the tendons. Some cases might be due to the extensor muscles not having tone enough to keep the bones in their proper vertical position, but most were due to the horse voluntarily relaxing his muscles and allowing the knee to go forward as a position in which painful feet were the least painful. The tendons then shortened to adapt themselves to the altered angles of the bones. Tired extensor muscles and tenderness in the feet were the chief causes of bent leg. In reply to Mr. Hopkin's remark, that he thought he had seen cases where the hock and stifle joints did not move in exact unison, he could only say that when that happened there must have been some defect in the connecting structures. In support of the practical axiom—always examine the foot—Professor Dick's name had been mentioned, and an old tale retold of his wonderful skill in diagnosing foot injury in addition to other and more patent lesions. Mr. Hunting said he was willing to allow Professor Dick very great acuteness, but he could not allow that a happy guess should be accepted as a sort of intuitive talent. It did much harm to inculcate the idea that lameness was diagnosed by some practitioners with preternatural sharpness, and that their diagnosis was founded on real observation and quick deduction. We all make mistakes, we are all very often puzzled, and if we do not confess our ignorance, we should not confuse the issue by classing together lucky guesses and careful diagnoses. It is the man who knows most and who sees clearest that most frequently feels that his opinion on a case of lameness is not too surely founded.

A cordial vote of thanks was accorded to Mr. Hunting for his very able and instructive paper.

The following office-bearers were elected for the ensuing year:—President, Mr. A. Leather; Vice-Presidents, Messrs. Hugh Ferguson and A. Michaelis; Treasurer, Mr. Tedbar Hopkin; Secretary, Mr. G. Gartside Mayor; Auditors, Messrs. John and Alex. Lawson.

A hearty vote of thanks to the retiring Chairman concluded the meeting.

W. F. Shore, M.R.C.V.S.,
Secretary pro tem.

Veterinary Jurisprudence.

CLAIM FOR ALLEGED INJURY TO A HORSE.

Bristol County Court.—Snow & Co. *v.* Cooksley.

Before his Honour, Judge Metcalfe, Q.C.

This was an action to recover the sum of £25 for injuries alleged to have been caused to a horse at the defendant's shoeing forge.

Mr. Vachell (instructed by Messrs. Benson and Carpenter) was for the plaintiff, and Mr. J. H. Clifton appeared for the defendant.

Mr. Vachell said the action was to recover damages for injuries sustained by a horse belonging to the plaintiffs, Messrs. Snow and Co., coal merchants. The horse was sent in July to the shoeing forge of the defendant to be shod, and on his return he developed symptoms of lameness, which gradually became worse.

Mr. Rumboll, veterinary surgeon, was eventually called in, and he expressed an opinion that the animal had been very badly pricked in process of shoeing. Remedies were applied, and the horse was kept from work. The animal appeared to get better, but directly it was put to work the lameness returned. The horse was about eight or ten years old, and up till now had always been a staunch and sound one.

Mr. Williams, horsekeeper, in the employ of the plaintiffs, deposed to sending the horse to be shod at the defendant's forge. When the horse came back it was lame. It was sent back to the defendant, but it was lame when it returned, and had been lame ever since. The animal was a staunch one, and was worth about £40 or £50. It had always done good work before, but since the lameness it had not done a day's work. As soon as the attempt was made to work it the lameness became very bad.

Several other witnesses were called, and Mr. Henry Rumboll, veterinary surgeon, deposed to examining the horse in question. He found traces of a nail-hole towards the heel, from which the nail had evidently been withdrawn. The hole was full of inflammation, which began to exude directly the searcher was applied. The wound, in his opinion, was caused by a nail, which had penetrated the quick.

Mr. Vachell said that concluded his case. In reply to his Honour Mr. Vachell said that he did not propose to call Mr. Jermyn—who it had been mentioned had examined the horse—because he was only called in recently, and then said he could not express an opinion on the point.

His Honour said he was not satisfied that the horse was pricked, and therefore judgment would be for the defendant.

NEGLECTING TO REPORT SHEEP-SCAB.

Official Veterinary Inspector's Confirmation required.

George Jenks, farmer, of Highley, Bridgnorth, was recently summoned for exposing fifteen sheep whilst suffering from scab. Inspector Jones visited defendant's farm on December 10th, and examined several flocks of sheep, finding fifteen animals suffering badly from scab.

For the defence it was contended that no conviction could lie under the Act unless the inspector gave information to a veterinary surgeon empowered to grant a certificate that such disease did exist. The only conclusive evidence on which the Bench could convict was a certificate from the veterinary surgeon, and as the prosecution had failed to produce any certificate that the

disease did exist the case should be dismissed. They were not to rely on any inspector's evidence unless it was supported by a certificate.

The Bench ruled otherwise, and, remarking that the case had been fully proved, fined defendant 30*s*. and costs. Mr. Haslewood (for the defendant) asked for a case for the higher court. The Act was most important to farmers, and as the point he had raised had never been decided, it was the wish of the farmers of the district that the section referred to should be decided by the higher court. The application was granted.

ADMINISTRATION OF ARSENIC BY HORSEKEEPERS.

At the Spalding Petty Sessions, five horsekeepers, named Baxter, Tansley, Atkin, Line, and Dunmore, in the employ of some of the principal farmers at Crowland, Lincolnshire, were prosecuted for administering arsenic to horses. Some eighteen horses had been treated with this poison, and it was stated that the practice—the object of which was to improve the animals' general condition—was becoming very general amongst horsekeepers in the locality, and that some valuable horses had died.

Evidence was adduced to show that mares were rendered of less use for breeding purposes if arsenic in small quantities was frequently given them. No defence was made by the men.

The Chairman said the defendants knew the practice to be illegal, and the magistrates were determined to put an end to it. They fined Baxter, Tansley, and Atkin £5 each, and Line and Dunmore were committed to prison for three weeks with hard labour.—*Agricultural Press.*

THE FITZWYGRAM PRIZES.

Lieut.-General Sir F. Fitzwygram offers his Veterinary Prizes for 1891 on the same, or nearly the same, conditions as in 1890.

OBITUARY.

It is with feelings of the deepest regret that we have to record the death of Mr. J. H. Steel, F.R.C.V.S., Principal of the Bombay Veterinary College, &c. The profession loses in Mr. Steel an energetic, intelligent, and earnest worker in veterinary literature and anatomical and pathological science. He was the son of a distinguished army veterinary surgeon, Mr. Charles Steel, now retired from active service. In due course he entered the Royal Veterinary College as a student, and passed his examination in March, 1875. Very shortly afterwards he was appointed Demonstrator of Anatomy, and having published

an excellent treatise entitled *Outlines of Equine Anatomy*, he was raised to the anatomical chair. After holding office for a few years he left for India, and soon started the *Quarterly Journal of Veterinary Science* in that country, becoming the chief editor. Ever ready with his pen, one publication was followed by another; thus in succession he published *Diseases of the Elephant*, *Bovine Pathology*, *Canine Pathology*, and lastly a treatise on the *Diseases of the Sheep*, 1890. Never very robust, although always very energetic, he suffered from the climate of India; and about three years since, in consequence of disease of the liver, he came to England and became a patient at Netley Hospital, an abscess with a fistulous opening having resulted from the malady.

The benefit he received was so great that he returned to India full of hope and confidence; but, alas! only for a short time, as his health having been so much undermined led to an early death, greatly to be lamented by his young wife and family and a numerous host of friends.

We have been asked to give insertion to the following letter, with which we cordially agree, and shall be ready to receive any donations that the members of the profession may see fit to entrust to our keeping. Few men more richly deserve to have their memory handed down to future members of the profession as an example than our departed colleague and friend.

The Animals' Institute,
9, Kinnerton Street,
Wilton Place, Belgravia, S.W.

JOHN HENRY STEEL.

To the Editors of the 'Veterinarian.'

Sirs,—The *Veterinarian* has always been so closely related to the Royal Veterinary College, that I feel certain no pages are so fit for an appeal to the profession to honour the work of the late Professor Steel, who first made his reputation at the Royal Veterinary College.

There also, as student and professor, he made a circle of friends, of whom I am one, who can never forget his genial ways and kindly help. His example and precept no one can trace the limit of or value, but his contributions to our literature are alone sufficient to indicate to those who did not know him personally what a debt the profession owes him.

There is a rumour that a memorial is to be established, and my object in writing this is to express a hope that it will soon assume some tangible form, and that the profession will respond generously to any appeal that may be made.

Yours truly,
John Atkinson, F.R.C.V.S.

February 14th, 1891.

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